THE DEVELOPMENT OF U.S. FLEET BALLISTIC MISSILE TECHNOLOGY:

POLARIS TO TRIDENT

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Declaration

I declare that this thesis is my own composition, the work of which has been carried out by myself, unless otherwise stated, and that it has not been previously submitted towards a higher degree or qualification.

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Some parts of this thesis include material which has already been published (or is in press) in D. MacKenzie and G. Spinardi, 'The Shaping of Nuclear Weapon System Technology: US Fleet Ballistic Missile Guidance and Navigation', in two parts, Social Studies of Science, Vol. 18 (1988).

Abstract

The main aim of the study is to document the development of US Fleet Ballistic Missile technology from its mid-1950s beginnings through to Trident II D5. This historical documentation is framed by a perspective which seeks to understand how technology evolves and what the relationship is between, to put it simply, technology and strategy, or technology and politics. Of particular interest in this case study is the relationship between technology and nuclear strategy. It is a commonplace assertion that technology is a dominant, determining factor in the arms race, that indeed there is a technological imperative. In particular there are many who argue that improvements in missile accuracies have driven changes in nuclear strategy away from counter-city retaliatory deterrence to war-fighting counter-force postures. Tracing the history of FBM development from Polaris, considered by many the archetypal counter-city deterrent, to Trident II, with hard-target kill capability comparable to MX, helps our understanding of this issue.

In considering this central theme, the development of FBM technology is analysed in the social constructionist terms of the 'new' sociology of technology. This approach argues that technical change must be explained impartially and symmetrically, and that the success of a particular technology is not sufficient explanation in itself, but is rather exactly what needs to be explained. Technology is considered to be underdetermined by the physical world, and thus to be fundamentally shaped by the social world.

The extreme characterizations of the relationship between technology and politics - either that technology is simply the tool of political will or that technology is out-of-control (as in the view that accuracy improvements have driven strategy) - are found to be inadequate in this study. Instead it is found that the 'bureaucratic politics' approach captures much of the rich complexity of the process of technological change. Yet even this approach fails fully to capture the complex inter-relatedness of 'technology' and 'politics', nor does it take into account the importance of the physical production of technology.

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Chapter 1

Introduction - The Sociology of Weapons Technology

Nuclear weapons pose an unprecedented threat to human civilization and the ecological health of our planet. Use of even a fraction of current arsenals would cause massive devastation and millions of deaths. Yet they also seem to be integral to the defence policies of some of the nations that possess them - in particular the United States and the Soviet Union.

Given this pervasive influence and potential danger there can be few more urgent tasks facing students of technology than analyzing the dynamics of nuclear weapons development. Understanding the course taken by the development of weapons technology is crucial if measures are to be taken to ensure the lowest risk of nuclear war. In particular, without some foresight, arms control measures will continue to be reactive, forever attempting to shut the stable door on horses that have long since bolted.

Technology and the Nuclear Arms Race

Post World War II international relations have been characterized by rivalry in nuclear weapons. In particular a central Cold War antagonism between the USA and USSR has accompanied their acquisition of a combined total of some 50,000 nuclear weapons. However, quantitative additions to arsenals are not the most worrisome feature of this 'arms race'. Quantitative limits and reductions are relatively easy to negotiate and verify, and small numerical imbalances are not of much 'military' significance at the high levels in question.[1] More disturbing are the continuous qualitative 'improvements' in nuclear weapons technology, which are more difficult to curb with arms control and more threatening to strategic stability.

1. Though much political capital can be made out of small numerical differences.

The main concern is that new technological developments may increase the risk of nuclear war breaking out during a crisis. That is, they may reduce *crisis stability*.[2] In particular, technologies which make a pre-emptive attack appear more feasible technically, such as improvements in missile accuracy, may increase the temptation to strike first during a serious crisis.

Nuclear-armed ballistic missiles are central technologies in the nuclear confrontation. Their relatively short flight time for 'strategic' use allows only the briefest possible tactical early warning of imminent attack. With flight times shorter than 30 minutes ballistic missiles have raised the pressing concern of pre-emptive nuclear attack by one superpower on the other.

Ballistic missiles now dominate the strategic arsenals of the superpowers, carrying some 70% of US warheads and 90% of Soviet warheads. These arsenals have grown quantitatively since ballistic missiles were first deployed in the late 1950s, but the most significant changes, especially recently, have been qualitative. Advances in technology appear to have altered radically the capabilities of ballistic missiles, and thus to have opened up new ways of using them.

Of particular importance are the development of multiple warhead technology and improvements in ballistic missile accuracy. The ability to carry several independently targetable warheads on one missile allows a greater 'exchange ratio' thus considerably adding to the potential effectiveness of a pre-emptive attack.[3] Coupled with increasingly better accuracy - itself a much greater contributor to effectiveness against hardened targets than extra explosive yield[4] - this has marked a general trend in the ballistic missile forces of both the USA and USSR towards

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2. Another concern is that technological advances may also spur on superpower rivalry, thus undermining arms race stability.

3. If restricted to single warhead missiles, neither of two sides possessing roughly equivalent missiles forces could expect a first strike to achieve high success. With reliability inevitably something short of a hundred per cent, the side firing first would need to expend all its missile force in order to destroy only a portion of the other's.

4. To obtain the same increase in a missile's destructive effectiveness against a hardened target as that achieved by a doubling of accuracy would require an eightfold increase in explosive yield.

greater hard target kill capability. These changes in technology have paralleled changes in nuclear strategy which have increasingly emphasized counterforce targeting, and in particular the destruction of hardened targets such as missile silos and command posts.

Many have seen this as a distinctive shift from a policy of deterrence based on the threat of retaliation against cities to a more unstable situation where the apparent ability to implement an 'effective' first strike (against fixed, land-based targets) will soon be technically available.[5] Some see the shift as actively desired, indeed the result of a 'secret agenda',[6] whereas others, more typically, attribute it simply to the inevitable, on-going advance of technology.

Thus Fred Halliday states that 'the possibility of greater accuracy in targeting missiles *led to* the shift from the "countervalue" approach, aiming at cites and economic targets, to one aimed at specific military targets, i.e. "counterforce"'.[7] But can technology be held responsible for this change in nuclear strategy? Indeed what is the relationship between technology and strategy?

Technology-out-of-control or Politics-in-command?

Attempts to characterize technological change in weaponry are bounded by two extreme positions - technology-out-of-control and politics-in-command - exactly paralleling the dichotomy in industrial innovation studies between technology-push and market-pull. At one pole it is held that technology is out-of-control, and that technological developments are a driving force in the formulation of strategic doctrine. Technology is seen

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5. The difficulties involved in locating and destroying missile-carrying submarines (SSBNs) make a complete first strike capability apparently unobtainable in the near-future, although the Soviet Union's large emphasis on ICBMs, low SSBN alert rate and its geographic encirclement make it seem most vulnerable in this respect. For a good survey of strategic anti-submarine warfare, see Donald C. Daniel, Anti-submarine Warfare and Superpower Strategic Stability (London: Macmillan/International Institute for Strategic Studies, 1986).

6. See, in particular, A. Roberts, 'Preparing to Fight a Nuclear War', Arena, No. 57 (1981), 45-93; reprinted in D. MacKenzie and J. Wajcman (eds), The Social Shaping of Technology (Milton Keynes: Open University Press, 1985), 279-94.

7. F. Halliday, The Making of the Second Cold War (London: Verso, 1983), 225, emphasis added.

as having its own 'internal', autonomous logic which makes it the dominant, determining factor in the nuclear arms race.

Such 'technological determinism' appears in many forms. At their most asocial there is the pervasive notion that technology is simply applied science, and that science is simply the physical world revealed. From such a characterization it is but a short step to viewing technical change as inevitable and monolithic in nature. Thus some appear to argue that technology possesses innate characteristics based on the 'laws of physics' that determine the pathways it will follow - that there are natural 'technological trajectories'.[8]

For example, Dietrich Schroeer argues that progress in computer capabilities 'may be a driving force producing a technological imperative towards improved missile accuracy'.[9] Technological imperatives, Schroeer claims, are the result of technologies 'so technically sweet and beautiful that they are difficult to resist'.[10]

Similarly Deborah Shapley has argued that the general onward advance of technology - 'technology creep', as she calls it - feeds back into military developments even when not specifically sponsored by them:

> What has happened is that the creep of technology - of the different technologies that bear on ICBM accuracy - has been advancing incrementally, cheaply, and with little public awareness...[11]

Another interpretation of technology-out-of-control places the emphasis not on the technology itself, but on the people and organizations that foster it. Rather than technology-out-of-control it holds 'technologists-out-of-control' to be responsible for the on-going pursuit of

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8. See R.R. Nelson and S.G. Winter, 'In Search of a Useful Theory of Innovation', Research Policy, Vol. 6 (1977), 36-76, though their view, while not clearly presented, does not appear to be quite as simple as this.
9. Dietrich Schroeer, 'Quantifying Technological Imperatives in the Arms Race', in D. Carlton and C. Schaerf, Reassessing Arms Control (Macmillan, 1985), 60-71.
10. Dietrich Schroeer, Science, Technology and the Nuclear Arms Race (New York: Wiley, 1984), 299.
11. D. Shapley, 'Technology Creep and the Arms Race: ICBM Problem a Sleeper', Science, Vol. 201 (22 September 1978), 1102-1105, at 1102.

advances in military technology. For example, Lord Zuckerman, former Chief Scientific Adviser to the U.K. Government, argues that:

> military chiefs ... merely serve as a channel through which the men in the laboratories transmit their views. For it is the man in the laboratory - not the soldier or sailor or airman - who at the start proposes that for this or that arcane reason it would be useful to improve an old or devise a new nuclear warhead; and if a new warhead, then a new missile; and given a new missile, a new system within which it has to fit. It is he, the technician, not the commander in the field, who starts the process of formulating the so-called military need.[12]

Similarly, Mary Kaldor considers that, at least in the US, military 'R&D has played an autonomous role in promoting the arms race.'[13] In particular, she claims, 'the organization of R&D institutions is the main factor which explains the impact of military R&D on the arms race'.[14]

Moving further still along the spectrum of views, one comes to theories of 'technological imperatives' which see technology not so much completely out-of-control, but instead in the control of a few, rather than society as a whole. Thus the development of weapons technology has been attributed to the powerful interests of a 'military-industrial-complex'. Not only technologists, or those involved in R&D, but also military and corporate actors are said to be caught up in a massive conspiracy to promote their own interests, and subvert the democratic political system, by ensuring the continuing development of weapons technology far beyond the nation's 'requirements'.[15] In this respect military technology may simply be seen as 'autonomous' due to the powerful vested interests which develop behind any large scale technological organization, as Langdon Winner has argued.[16]

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12. Lord Zuckerman, 'Science Advisers and Scientific Advisers', Proceedings of the American Philosophical Society, Vol. 124 (1980), 241-255, at 250-51.
13. Mary Kaldor, 'Military R&D: cause or consequence of the arms race?', International Social Science Journal, Vol. 35, No. 1 (1983), 25-45, at 26.
14. Ibid, 42.
15. A variant of this approach can be found in 'Marxist' explanations of technical change which seek to implicate 'class struggle' as the motor behind technical change, though here technology is clearly seen to be only out of the control of those it is being used to exploit.
16. L. Winner, Autonomous Technology: Technics-out-of-Control as a Theme in Political Thought (Cambridge, Mass.: MIT Press, 1977).

Finally, at the opposite end of the spectrum from technology-out-of-control is politics-in-command, whereby political elites are said to be able to consciously shape the technology they desire. Politics-in-command would be the official viewpoint taken by members of the US military establishment. Weapons are developed, it would be claimed, to satisfy a national requirement which would have been politically decided by the Administration and Congress. National governments are thus seen as rational actors developing technologies in pursuit of goals which they expect the technologies to help them satisfy. Developments in nuclear weapons technology would normally be argued to be necessary tools in order to achieve the desired deterrence of the Soviet Union.

But critics of official policy have also taken the 'politics-in-command' viewpoint. Thus, argues Roberts, the development of counterforce technology has been deliberately pursued by the superpowers because of the perceived political benefits of such a capability. In so arguing, Roberts specifically seeks to refute the view that: 'Military technology is out of control, developing autonomously and dragging military thought behind it'.[17]

However, dissatisfaction with rational actor analysis of international relations - which views states as though they were unitary actors able to respond rationally to, say, the Soviet 'threat' - has also led to another approach which assigns primacy to politics in the formulation of policy, as well as in the development of technology. But rather than the products of rational decision-making these are seen as the contested outcome of organizational and bureaucratic conflict and accommodation.[18] In this 'bureaucratic politics' model technology is seen as an outcome of the many turf battles, compromises and wrangles which particularly predominate in the pluralistic US political system.[19]

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17. Roberts, 279.

18. A classic account of the bureaucratic politics approach is G. T. Allison, Essence of Decision: Explaining the Cuban Missile Crisis (Boston: Little, Brown & Co., 1971).

19. Major works adopting this approach are Harvey Sapolsky, The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972) and E. Beard, Developing the ICBM: A Study in Bureaucratic Politics (New York: Columbia University Press, 1976).

Recent Approaches to Technical Change

Recent work in the sociology and history of technology has suggested other ways of looking at technical change and the relationship between technology and its social/political environment.[20] Two distinct, though closely related, approaches can be identified: 'the social construction of technology' and 'systems' or 'actor-network' building.

Pinch and Bijker have proposed that the 'social construction of technology' can be analysed in the same relativist manner adopted in the 'strong programme' of the sociology of scientific knowledge.[21] Technology, they argue, like science is underdetermined by the physical world, and needs to be explained by reference to social factors. Success and failure must be analysed in an impartial and symmetrical manner, without any reference to 'truth': 'The success of an artefact is precisely what needs to be explained. For a sociological theory of technology it should be the *explanandum*, not the *explanans*.'[22]

Pinch and Bijker seek to explain technological outcomes in the same way that the sociology of scientific knowledge explains the 'closure' of scientific disagreements.[23] Firstly, the technological or scientific issue in question is shown to display 'interpretative flexibility' - for example, that there is more than one way to design an artefact, or that more than one conclusion can be drawn from a particular set of experiments. Such conditions are usually apparent only during scientific controversy or in the early stages of the development of a technology, but are always there in principle. A consensus will usually form around one design or one interpretation. This 'closure' is effected by social mechanisms, not simply compelled by logic, or rationality, or efficiency, or whatever.

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20. See Wiebe E. Bijker, Thomas P. Hughes and Trevor Pinch, The Social Construction of Technological Systems, (Cambridge, Mass.: MIT Press, 1987).
21. T. J. Pinch and W. E. Bijker, 'The Social Construction of Facts and Artefacts: or How the Sociology of Science and the Sociology of Technology might Benefit Each Other', Social Studies of Science, Vol. 14 (1984), 399-441.
22. Ibid, 406.
23. Ibid, 409.

However, whereas one dominant group, the 'Core-Set', is generally the locus of closure in science, most technology is necessarily involved with many social groups. Pinch and Bijker therefore argue that it is necessary to identify the 'relevant social groups' for the technology in question.[24] By adopting a symmetrical and impartial view of their relations to the technology, it should be possible to explain how interpretative flexibility undergoes social closure to leave a technology which is considered successful or failed.

This approach has been criticized as appearing to unduly favour the 'relevant social groups' in their relationship with technology whilst also artificially separating the two. It suggests that 'the social lies *behind* and directs the growth and stabilization of artefacts'.[25] In the other main strand of sociological thinking about technology, successful technological developments are seen as systems held together through the building of networks.[26]

One main contributor to this viewpoint is an historian, not a sociologist. In the work of Hughes, human system builders, such as Edison, are seen to be skillful manipulators, not only of 'technical' and scientific detail, but also of the economic, political and legislative environment which impinges on their system.[27] These system builders, Hughes argues, advance technological systems by identifying 'reverse salients', parts of the system which are perceived to be holding up the rest, upsetting the system's harmony and impairing its growth or performance (usually judged in economic terms). The reverse salients are generally obvious to the system builders, who then proceed to define 'critical problems' which, when solved, will remove the reverse salient. For example, Edison worked out that a high resistance filament was necessary for an electric light because it would allow the use of thinner power cables

24. Ibid, 414.
25. J. Law, 'Technology and Heterogeneous Engineering: The Case of Portuguese Expansion', in Bijker, Hughes and Pinch, 111-34, at 113.
26. Boelie Elzen, Scientists and Rotors: The Development of Biochemical Ultracentrifuges (The Haag, Netherlands: Cip-Gegevens Koninklijke Bibliotheek, 1988), 5-7 considers the 'systems' and 'network' approaches as sufficiently different to be considered separately, but this seems more a reflection of their differing origins and terminology than of anything substantive.
27. This section draws particularly from T. P. Hughes, Networks of Power: Electrification in Western Society (London and Baltimore, MD.: The Johns Hopkins University Press, 1983).

and reduce the capital outlay on copper, so ensuring that electric lighting would be economically competitive with the established gas lighting.[28]

In his study of the growth of electric power systems, Hughes identifies several chronological phases. At first, the system builders are inventors and entrepreneurs, attempting to build a working system. Once the system is established, however, growth and efficiency become more important, and financiers and trained engineers begin to dominate. Later still, when the system reaches a mature phase, growth stabilizes and the manager and consulting engineer achieve preeminence. Whereas the original system builders were interested in changing the world with their technology, in the mature phase their prime concern is simply to maintain the status quo.

Others have also taken a 'systems' approach to explaining the development of technology. Law has coined the phrase 'heterogeneous engineer' to describe what effective system builders need to be when 'attempting to build a world where bits and pieces, social, natural, physical or economic, are interrelated and *keep each other in place* in a hostile and dissociating world'.[29] Like Hughes, Law does not see social factors as necessarily dominant: 'Other factors - natural, economic, or technical - may be more obdurate than the social and may resist the best efforts of the system builder to shape them.'[30] According to Latour, the task facing the heterogeneous engineer is to 'make your environment such that whatever other human or non-human actors think or do, they are either kept at bay or else they help strengthen your position, making the world safer, more predictable and more enjoyable for you.'[31]

To do this, Law, Latour and their collaborator Callon argue, the heterogeneous engineer must 'translate' the interests of disparate actors in

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[28]. T. P. Hughes, 'The Electrification of America: The System Builders', Technology and Culture, Vol. 20, No. 1 (1979), 124-161, at 135-7.
[29]. J. Law, 'On the Social Explanation of Technical Change: The Case of the Portuguese Maritime Expansion', Technology and Culture, Vol. 28, No. 2 (1987), 227-252, at 231, emphasis in original.
[30]. Law, 'Technology and Heterogeneous Engineering', 113.
[31]. B. Latour, '*The Prince* for Machines as well as for Machinations', in Brian Elliott (ed.), Technology and Social Process (Edinburgh: Edinburgh University Press, 1988), 20-43, at 29.

order to build the desired technological network.[32] They must enrol other actors' support without at the same time surrendering control over the nature of the technology produced. Moreover, technology does not simply 'diffuse' once developed, but is constantly subject to the shifting efforts at translation that necessarily accompany it through its use and modification.[33]

In the systems or network approach there is no attempt to decide whether 'technical' or 'social' factors are dominant. Any such distinction is simply considered artificial and unhelpful. But whereas Law and Latour, for example, regard constant heterogeneous engineering as vital to keep systems together, Hughes emphasizes the persistence of technological systems with his notion of 'technological momentum'. This he has defined as 'loosely connected, mutually reinforcing components that constitute a system of vested interests involving people, institutions, ideas, and artefacts ... that tends to resist change and softly determine the course of events.'[34]

The Empirical Study - US Fleet Ballistic Missiles

These concepts seem to be useful aids in a study of the development of weapons technology, and of the relationship between technology and strategy. Some previous studies of weapons technology have tackled this issue. However, almost without exception they have the general failing of limiting their scope to the time span of a single weapon system. Technical change is thus frozen in time, and technology appears as a static given - a resource that can be drawn on - rather than a dynamic part of the system.

This study deliberately focuses not on a single generation of a weapon system, but on the evolution of fleet ballistic missile (FBM) technology over a period of over thirty years. (Some of the main features of US FBMs are summarized in Figure 1.1.) By tracing the parallel

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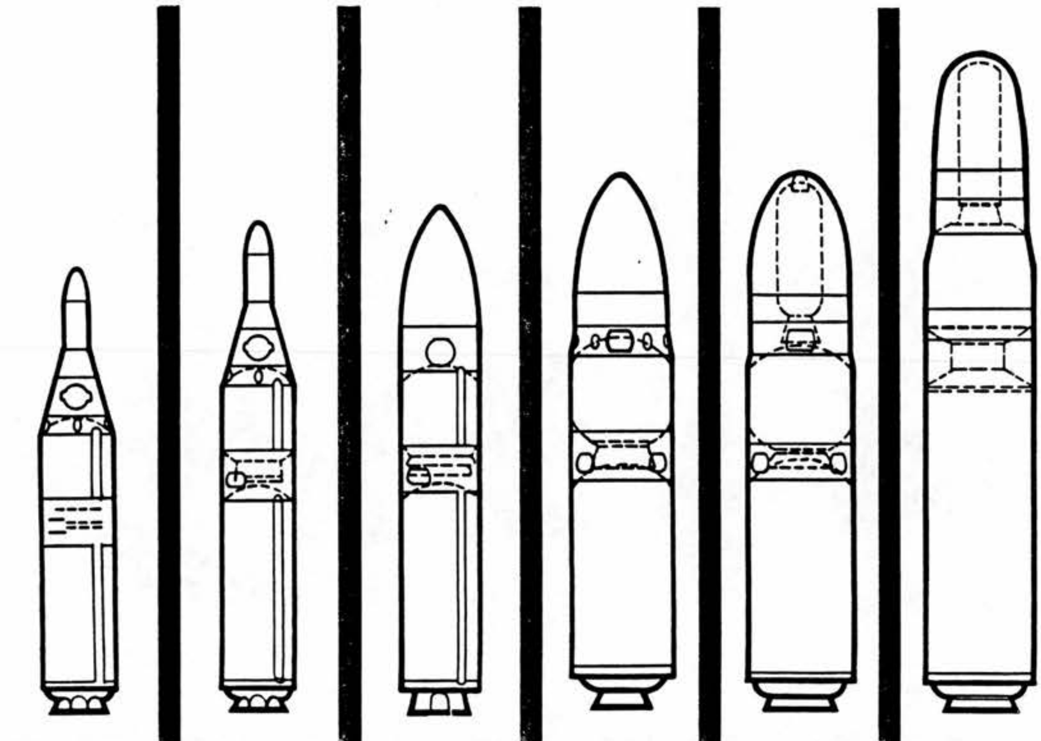
32. See, in particular, B. Latour, Science in Action (Milton Keynes: Open University Press, 1987), 108-132.
33. Ibid, 132-44.
34. Quoted in Alex Roland, 'Technology and War: A Bibliographic Essay', in Merritt Roe Smith (ed), Military Enterprise and Technological Change (Cambridge, Mass.: MIT Press, 1985), 378, fn 220.

development of technology and nuclear strategy during this time it is hoped that a more sophisticated understanding of their interaction can be obtained.

The shift in missile technology and targeting rationale towards counterforce is particularly evident in the US Navy's Fleet Ballistic Missiles. The original Polaris, first deployed in 1960, seemingly provided the ideal deterrent, able to remain submerged and invulnerable at sea and capable of little other than deadly retaliation against Soviet cities as a last resort. Due for deployment some thirty years later, the latest FBM, Trident II, is claimed to have a combination of accuracy and explosive yield which makes it comparable to the Air Force MX in its high likelihood of destroying hardened targets.

This shift provides the central focus of this study, which will describe the evolution of those parts of FBM technology that most generally relate to the system's perceived strategic capability. It is not possible to cover every aspect of the development of FBM technology here. Instead some technologies - such as navigation and guidance - will play a much greater part in the story than others because of their greater strategic significance.

These central changes in the technology and strategic role of the FBM system are documented chronologically in chapters two to seven. Although not following any theoretical perspective rigorously the narrative of these chapters implicitly uses ideas drawn from the sociology of technology. In constructing this history the empirical material will be generally presented in a *systems* framework. Emphasis will be placed on the *heterogeneous engineering* needed to build and maintain the FBM system. At the same time explanations of technological developments will be guided by *empirical relativism.* What it means for a technology to 'work' will be examined critically as an explanation of success.



	A-1 POLARIS	A-2 POLARIS	A-3 POLARIS	C-3 POSEIDON	C-4 TRIDENT	D-5 TRIDENT
Length (feet)	28.5	31.0	32.3	34.0	34.0	45.8
Nominal Range (nautical miles)	1200	1500	2500	2500-3200	4000	4000+
Weight at launch (1000s of lbs)	28.8	32.5	35.7	65.0	73.0	c130.0
Year first deployed	1960	1962	1964	1971	1979	1989
No. of warheads	1	1	3 [MRV]	average of 10 [MIRV]	8 [MIRV]	10-14 [MIRV]
Yield per warhead (kilotons)	600 [W47]	800 [W47]	200 [W58]	40 [W68]	100 [W76]	475 or 100 [W88 or W76]
Guidance System	Mk1	Mk1	Mk2	Mk3	Mk5	Mk6
Approximate circular error probable (nautical miles)	2	2	0.5	0.25	0.12-0.25	0.06

Sources
General data and diagram from FBM facts/chronology - POLARIS, POSEIDON, TRIDENT (Washington, DC: Strategic Systems program Office, 1986) and earlier editions.
Accuracy and warhead yield figures are officially classified and have been deduced from a number of other sources: T. B. Cochran et al., Nuclear Weapons Databook, Vol. 1: US Nuclear Forces and Capabilities (Cambridge, Mass.: Ballinger, 1984); W. M. Arkin, 'Sleight of Hand with Trident II', Bulletin of the Atomic Scientists, Vol. 40 (December 1984), 5-6; R. S. Norris, 'Counterforce at Sea', Arms Control Today, (September 1985), 5-12.

Figure 1.1: US Fleet Ballistic Missiles

However, the historical nature of the study presents some difficulty here. As sociologists of science have found, it is not easy to recover interpretative flexibility once closure has occurred. Likewise many of the historical accounts of FBM technology provide little sense of alternative approaches that are long forgotten. Nor do participants tend to remember in detail the potential malleability of technology, except for the few deeply contested instances. The social nature of technology can thus seem neglected as 'purely technical' explanations of success dominate the historical record. Where things are described as technical in the following this refers to the beliefs of participants, and their definitions of what 'technical', 'political', 'strategic' or 'economic' mean, and should not be taken as implying that technology can be usefully explained monocausally. Indeed it is exactly to avoid such over-rigid explanations that no single theoretical perspective is used to constrain the historical narrative, but instead the empirical material is allowed to 'speak for itself'.

In the final chapter the different concepts concerning the role of technology in the arms race, and different theories of technical change, are discussed in the light of the case study. In addition, the implications of such studies of weapons technology for arms control are considered.

A Note on Sources

As well as the open literature, which is extensive, and some archival material, this study draws heavily on interviews with present and former participants in the FBM programme. A full list of those interviewed is given in the Appendix, but interviewees are not cited by name in the footnotes. No source material, whether it be an interview, archival document or published article has simply been accepted uncritically at face value. In attempting an explanation of technology which takes care to understand the role of social factors, it would be naive to ignore their role in the way people write or speak about technology!

In addition to 45 interviews carried carried out directly for this study it has also been possible to draw on some other related interviews carried out by Donald MacKenzie in his work on inertial guidance and navigation technologies. These are also listed in the Appendix.

Interviews were arranged simply by writing to or telephoning the relevant individuals. Once a few key people and organizations had been identified, others 'mushroomed' quickly. Simple lack of time meant that it was not possible to interview everyone. However, those interviewed include most of the 'core-set' of major participants in the FBM programme. I am particularly grateful to the Strategic Systems Program Office of the US Navy for their cooperation in arranging interviews (and to Andrew DePrete who was my contact there), as well as to the other organizations and individuals who were helpful.

In these interviews no attempt was made to gain access to classified information, and the study as a whole is based solely on unclassified (and declassified) sources. Perhaps surprisingly this is not an insurmountable obstacle to writing a technical history of a nuclear weapons system programme. Much technical information is not classified, and where quantitative details are so, it still remains possible to gain adequate qualitative descriptions.

Considerable technical detail can also be found in the open literature, especially in journals such as *Aviation Week & Space Technology*, and for the early period of FBM development, *Missiles & Rockets*. These and other historical accounts have an unfortunate tendency, however, to construct a dichotomy between the 'technical' on one hand and the 'political' or 'social' on the other. Technical accounts are overwhelmingly of the 'B followed A because it was better' variety, in which the social world enters only rarely. Accounts by political scientists, on the other hand, tend to treat the technology largely as a black box, the content of which is not considered especially important.

Nevertheless, although in this vein, Harvey Sapolsky's book remains an excellent source of information on Polaris.[35] Ted Greenwood's account of the development of MIRV technology not only provides one of the best interminglings of the technical and political, but also the best description of the origins of Poseidon.[36] The third book-length account by political scientists of the FBM programme, Dalgleish and Schweikart's discussion of Trident, is less helpful.[37] Numerous other pieces of academic and indeed journalistic writing also provided useful sources of information. Finally, a rich source of information (though very time-consuming to use) lies in the various Congressional hearings. Most useful for this study have been hearings from the Senate Armed Services Committee Subcommittee on Research and Development, particularly during the 1970s. (To save repetition, hearings from this committee will be abbreviated to SASC in the footnotes).

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35. H. M. Sapolsky, The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972).

36. T. Greenwood, Making the MIRV: A Study of Defense Decision Making (Cambridge, Mass.: Ballinger, 1975).

37. D. Douglas Dalgleish and Larry Schweikart, Trident (Carbondale, Il.: Southern Illinois University Press, 1984). For a review, see G. Spinardi, 'Trident: Tracing the Course of Nuclear Weapons Technology', Social Studies of Science, Vol. 17 (1987), 371-81.

Chapter 2

Heterogeneous Engineering and the origins of the Fleet Ballistic Missile.

I don't care how big and ornery it is, we're going to take the bastard to sea.

Admiral Raborn.[1]

Polaris was not simply the coming together of several ripe technologies, the inevitable outcome of technical progress. Nor did it just appear ready-made in response to a national call to arms. In retrospect a submarine-launched ballistic missile seems an obvious enough technology, providing as it does a method of basing nuclear-armed missiles that is relatively invulnerable, both to enemy attack and to domestic protest. However, in the early 1950s it was far from obvious that ballistic missiles capable of carrying nuclear warheads over the desired range could be deployed in submarines.

The Wrong Stuff

Indeed if any missile was going to carry nuclear warheads from submarines, or anywhere else for that matter, to the Soviet Union, it was the conventional wisdom in the decade following World War II that it would be a cruise missile not a ballistic one. In the USA dominant opinion considered ballistic missiles to be the more difficult technology, something to be considered in, say, twenty years time, when the

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1. Cited in James Baar and William E. Howard, Polaris! (New York: Harcourt, Brace & World, 1960), 52.

technology had matured.[2] For the time being, cruise missiles, analogues of the German V1 rather than the V2, were thought most promising.

Both types of missile had, of course, been brought to fruition as weapons by the Germans. The V1 'doodlebug' was essentially a pilotless aircraft powered by a jet engine capable of carrying a one ton warhead a distance of about 150 miles. Some 8000 were used during the war, mainly against Southern England. The V2 was a liquid-fueled rocket with sufficient thrust to lift it to an altitude of about 50 miles from which it plummeted ballistically on a parabolic flight path to impact.

As Germany was overrun, the Allies scrambled to capture the V weapon technology - both the hardware and the know-how of the German engineers. Most went to the USA, where the Armed Services each had their different approach. The Army Ordnance Corps was already sponsoring work which had led to the invention in 1943 of the Corporal E at Caltech's Jet Propulsion Laboratory.[3] After the war, in project Hermes, the Army used the captured V2s and the German missile engineers and scientists to carry out high-altitude research.[4]

Whereas Ordnance favoured a short-term approach to a military missile, making the best of what technology was available, the Army Air Forces wanted one of intercontinental range. In 1946 the Air Forces sponsored a 5000-mile range ballistic missile, extrapolated from V2 technology. However, it was soon agreed that such a missile was at least a decade away and the Air Forces dropped the development, deciding instead to concentrate on long-range cruise missiles. Towards the end of the war a semi-formal division of responsibility was agreed between

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2. The reasons for this in the US Air Force are considered in R. Perry, The Interaction of Technology and Doctrine in the USAF (Santa Monica, Calif.: The RAND Corporation, January 1979; P-6281), esp. 8-15.

3. See Clayton R. Koppers, JPL and the American Space Program (New Haven: Yale University Press, 1982).

4. W. Lucas, 'Political Bugs' in 'Rocketry in the 50s', Astronautics and Aeronautics, Vol 10, No 10 (Oct 1972), 44.

Ordnance and Air Forces, with Ordnance given jurisdiction over ballistic missiles and Air Forces over aerodynamic designs.[5]

The Navy too, initially investigated the V2 technology, and on September 6, 1947 launched one from the aircraft carrier *Midway*.[6] In addition enthusiasts within the Bureau of Aeronautics (BuAer) secured Navy support for the development of a satellite launch system, a liquid-fueled rocket called Viking. This also demonstrated the feasibility of launching a missile from a ship, and in 1952 spawned a proposal for a 500-mile range military version.[7] This, however, was vetoed by the director of the Navy's guided missile program, Captain Sides, and by the Chief of Naval Operations.

Cutbacks in Navy funding ensured strong resistance to spending money on new, unproven technology. Moreover, in Operation Pushover in 1949 the Navy had investigated the effects of an accident to a V2 on a mock-up ship. The damage caused by the liquid fuel explosion left a long impression: 'One look at that mess, and a shudder ran through every ship in the Navy'.[8] Instead the only long-range missiles developed by the Navy were again of the aerodynamic cruise missile type. Postwar tests of improved V1s, known as Loons, had been successful enough to lead the Navy to begin developing its own cruise missile, the 575-mile range Regulus, in 1948.[9] This was the responsibility of the Navy's Bureau of Ordnance, which, so committed, gave little support to BuAer's pressure for ballistic missile development.[10]

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5. Robert Perry, The Ballistic Missile Decisions (Santa Monica, Calif.: The RAND Corporation, 1967; P-3686), 4fn.

6. W. D. Miles, 'The Polaris' in E. M. Emme (ed.), The History of Rocket Technology: Essays on Research, Development and Utility (Wayne State University Press, 1964), 163.

7. Vincent Davis, The Politics of Innovation: Patterns in Navy Cases, the Social Science Foundation and Graduate School of International Studies Monograph Series in World Affairs, IV, 3 (Denver, Colo.: University of Denver Press, 1967), 33.

8. Baar and Howard, 14.

9. Captain Dominic A. Paolucci (US Navy, Retd), 'The Development of Navy Strategic Offensive and Defensive Systems', United States Naval Institute Proceedings, Vol. 96 (May 1970),204-223, at 210.

10. Ibid.

Thus up until 1954 research and development on long-range ballistic missile technology languished in the USA.[11] Cruise missiles, which then really were just pilotless aircraft, seemed a natural evolution of current technology, compared to which ballistic missiles looked radical and distant. But far from being a 'natural technological trajectory', the preference for cruise over ballistic missiles stemmed more from the nature and expectations of the organizations involved. What seemed technically easier was also organizationally easier for the Navy and Air Force,* where ballistic missiles implied a radical change in roles.

For the Air Force pilotless aircraft required less getting used to; they could simply replace their manned analogues. For the Navy ballistic missiles seemed at this time to necessarily require the feared liquid fuel, but to offer much poorer accuracy than cruise missiles.[12] The expected low accuracy meant that ballistic missiles were considered to be of little use against 'targets of naval interest'. They could only be used for 'strategic' bombardment against cities, a role which was unpalatable to many in the Navy (who had criticized the Air Force's adherence to such a policy) and which would have led to unwanted rivalry over the strategic mission.[13]

Defining the FBM

But throughout the early 1950s the advocates of a sea-launched ballistic missile became increasingly convinced of 'technical' feasibility. To those working in the area, advances in solid propellant technology showed promising potential. There was a sense that 'even though solid rocketry was in ... its very early phases, we felt that by extrapolation we could see the feasibility of building a solid system that could do the job'.[14] Indeed the recollection of Admiral Levering Smith - a central figure in the

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[11]. For extensive documentation of this, see, in addition to Perry, 'Ballistic Missile Decisions', E. Beard, Developing the ICBM: A Study in Bureaucratic Politics (New York: Columbia University Press, 1976).
[12]. In 1955 the operational requirement for the Triton Navy cruise missile was a Circular Error Probable of 600 yards, whilst that of the proposed FBM was 4000 yards. B. D. Bruins, 'U.S. Naval Bombardment Missiles, 1940-1958: A Study of the Weapons Innovations Process' (Columbia University PhD Thesis, 1981), 285.
[13]. Bruins, 280.
[14]. Interview.
*, In 1947 the Army Air Forces became a third service, the U.S. Air Force.

development of Navy ballistic missiles - was that 'it was our conclusion at that time that the technology would reasonably support all the elements of such a system except for knowing where the launch platform was with sufficient accuracy.'[15]

But, of course, 'technical' feasibility alone neither brings new technology into being, nor defines its workability. What did lead to the creation of fleet ballistic missile technology had to do with the many wider concerns which are important to all technology, but which are rarely considered strictly technical. Most important was 'selling' the technological projections which was all the Fleet Ballistic Missile - as the Navy would dub their sea-based IRBM to highlight its differences from the land-based Jupiter and Thor[16] - constituted at the time.

The nature of ballistic missile technology (in common with many other large-scale technologies) makes this particularly striking. FBM advocates had to convince both the Navy and then the Administration to provide funding for a very expensive hypothetical technology based on extrapolations of what had so far been achieved. The only way to really 'know' if it 'worked' was to build it and see; by which time it would be too late to ask for the money back if it was considered a failure. FBM advocates were thus not 'selling' hardware, but rather concepts and a 'paper' system.

Lacking whole-hearted support within the Navy, FBM proponents might have been frustrated for many more years had it not been for the establishment of a special committee by President Eisenhower in the spring of 1954. Officially known as the 'Technological Capabilities Panel', the Killian Committee looked at the prospects for and significance of long range missile developments. Navy FBM advocates in BuAer - Captain Robert F. Freitag and Abraham Hyatt - channeled papers supportive of a Navy missile through the Killian Committee's Navy Department liaison representative, Commander Peter Aurand.[17]

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15. Interview.

16. According to one interviewee: 'the term fleet ballistic missile was used for political reasons. If they'd called it an IRBM, an intermediate range missile, it would then have been seen as competing with Thor and Jupiter.'

17. Davis, 34.

The Killian Committee's report, entitled 'Meeting the Threat of Surprise Attack', was presented to the National Security Council on February 14 1955. Amongst many other recommendations it gave acceleration of ballistic missiles a high priority, as had the Strategic Missile Evaluation Committee, headed by John Von Neumann, the previous year. What was different, however, was the emphasis which the Killian Committee placed on the urgent development of intermediate range ballistic missiles (IRBM). In their view, developing 1500-mile range IRBMs would be 'much easier and have much greater assurance of success' than relying entirely on building a 5000-mile ICBM.[18] Endorsement by the Killian Committee of the fleet ballistic missile concept, as noted in high-level papers circulated in early 1955, was then used to bolster support for it within the Armed Services. Significantly Freitag and Hyatt could now count on the backing of senior officers within BuAer, including the most senior of all, their Chief, Rear Admiral James S. Russell.

Nevertheless, substantial resistance to a FBM still remained in the Navy. Doubts about feasibility strengthened the position of those concerned about the opportunity costs. As Chief of Naval Operations, Admiral Carney, and the director of guided missile developments on his staff, Rear Admiral Sides, saw it, the technical requirements of a viable FBM system were some way from being satisfied. In short, they felt that there was no proven small warhead of adequate yield, no sufficiently accurate guidance system, no suitable fire control or navigation system, and no sufficiently powerful solid propellant. 'There wasn't even a concept as to a launching system', as Admiral Arleigh Burke later recalled.[19] Consequently when Admiral Russell sent a memorandum to the Chief of Naval Operations in July stating that the Bureau of Aeronautics was proceeding with the development of a ballistic missile, Admiral Carney's response was negative. He decided that no research and development should proceed on the FBM concept, and sent a letter

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18. Beard, 197.
19. Davis, 37.

directing BuAer to discontinue all efforts in this area, and to enter into no formal budget commitments or contractual arrangements.[20]

But it arrived too late. By then Freitag and Hyatt had already mailed out a letter to 22 aerospace contractors and defence research laboratories. The letter stated Freitag and Hyatt's FBM vision and asked for advice and suggestions. On the whole the responses were encouraging, and helped generate further support for the concept. At about the same time, Admiral Russell exercised his privilege as a Bureau Chief in by-passing the Chief of Naval Operations and appealing directly to the civilian Secretariat of the Navy. The Assistant Secretary of the Navy for Air, James H. Smith, was converted to the cause, and he, in turn, converted other influential figures. The importance of Smith's support was such, according to one observer, that without it 'the Navy would have probably missed forever the opportunity to develop and acquire fleet ballistic missiles'.[21]

Even so, considerable opposition to a Navy IRBM remained. In the summer of 1955 Deputy Secretary of Defense Ruben Robertson prepared a memorandum for Secretary of Defense Charles E. Wilson which recommended giving the Air Force a monopoly over IRBM development. Only the strong protests of the Navy Secretariat prevented the memorandum from being sent.[22]

Robertson had sought to exploit the Navy's internal divisions to limit the cost of missile development by excluding the Navy (as well as the Army) from long range missile work. But with the appointment of Admiral Arleigh Burke as Chief of Naval Operations on August 17 1955 the Navy would at last present a unified front on the FBM question.

Whilst preparing to take over as CNO during July Admiral Burke had visited the Heavy Electronics Division of the General Electric Company in Syracuse, where he was briefed on work done for the Air

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20. Ibid.

21. Ibid, 38.

22. Harvey M. Sapolsky, The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972), 21.

Force on ICBM guidance. He was told that the guidance systems could be adapted for a sea-launched ballistic missile if the Navy was prepared to sponsor such work.[23] Within twenty four hours of taking office he called for a briefing on the FBM concept. In less than a week he had made up his mind. The restrictions which Carney had placed on the Bureau of Aeronautics were lifted, and they were urged to make efforts to increase backing for the FBM within the Navy.[24]

Thus when the Killian report was endorsed by the National Security Council in September 1955, with both land and sea-based IRBMs to be considered, the Navy was at last in a position to put forward a positive proposal. Although still receiving advice from some of his staff recommending that the Navy should not give priority to an IRBM, Burke decided on 19 October to press ahead. At his instigation a widespread campaign was organized to ensure support for a Navy FBM.[25] Written assurance which seemed to say that IRBM costs would be allocated separately from the normal Navy and Army budgets helped quell internal Navy opposition to the project.[26]

However, by this time the President and Department of Defense officials had decided that, although getting the highest priority, ballistic missile programmes should be limited to four. Already the Air Force had three programmes approved - the Atlas ICBM; the back-up Titan ICBM; and the Thor IRBM - and because of their German engineers' experience it was felt that the Army Jupiter IRBM should be the fourth. Only if the Navy could find a partner would they be able to become involved in ballistic missile development at this critical stage. At first the Air Force was approached to see if it would be willing to allow a sea-based version of the Thor to be developed. Unhappy about the technical changes required to make the Thor adaptable to sea-launching and in no need of an ally the

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23. D. A. Rosenberg, 'Arleigh Albert Burke' in R. W. Love, Jr., The Chiefs of Naval Operations (Annapolis, Maryland: Naval Institute Press, 1980), 263-319, at 278.
24. Davis, 38.
25. Rosenberg, 'Arleigh Albert Burke', 278.
26. A November 1955 memo from Secretary of Defense Wilson said that: 'The dollar requirements for the IRBM program are separate from the dollar requirements or limitations applicable to any other Army and Navy program, and will be justified separately.' Quoted in Bruins, 287.

Air Force rejected the offer. The Navy then went to the Army with a proposal for a joint IRBM. Such a collaboration was 'technically' no more appealing to the Army than to the Air Force, but it seemed to offer better prospects of preventing Air Force hegemony over ballistic missile forces. In early November Admiral Burke and the Army's Chief of Staff, General Maxwell D. Taylor, agreed to the collaboration.

On 8 November 1955 memoranda were sent from Secretary of Defense Wilson to the Service Secretaries authorizing the development of IRBM at the 'maximum speed permitted by technology'. The whole IRBM programme was to consist of 'a land-based development by the Air Force (IRBM No.1) and a joint Army-Navy Program (IRBM No.2) having the dual objective of achieving an early shipboard capability and also providing a land-based alternative to the Air Force program'.[27]

Thus defined the FBM was an organizational compromise. Its nature was determined not by any clearly defined strategic role, nor by the technical preferences of FBM advocates, but simply by the 'need' to get a share of the ballistic missile 'pie'. Many in the Navy doubted that this was the correct course to take, either because they preferred to stick with the more evolutionary, 'easier' Regulus technology, or more generally because they feared the effect the financial drain of ballistic missiles would have on the Navy's traditional surface fleet roles: 'most of the senior officers in Washington, with the exception of Admiral Burke, were not deliriously happy to embark on such a risky and costly venture as this'.[28] Admiral Burke, however, believed that the Navy needed to take advantage of the new technology, and to compete with the Air Force for a share in the resources allocated to it.[29]

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27. M. H. Armacost, The Politics of Weapons Innovation: The Thor-Jupiter Controversy (New York: Columbia University Press, 1969), 71.
28. Interview with Vice Admiral W. F. Raborn by J. T. Mason, Jr. (15 September 1978), 17.
29. Rosenberg, 'Arleigh Albert Burke', 278.

The Special Projects Office

Burke moved quickly to consolidate the Navy's ballistic missile role. A new programme office was established on November 17. Simply named the Special Projects Office (SPO) this broke with a Navy tradition whereby procurement was the responsibility of various technical bureaus. Thus Burke avoided the difficult and divisive choice between the Bureau of Aeronautics and the Bureau of Ordnance, both of which now sought control of the FBM programme.[30] With a single programme office bureaucratic intrigues would be minimized. Burke's shrewdness also guided the choice of SPO's first director, Rear Admiral William F. Raborn, a naval aviator: 'I did not want a technical expert because a technical expert would be too narrow-minded. I wanted an aviator because if this missile were successful it would jeopardize the aviation branch.'[31]

Burke then gave Raborn the power necessary to assure the FBM of the highest priority in the Navy. A December 2 memorandum from Burke soon became known as Raborn's 'hunting license' as he used it to obtain the Navy's best technical officers and civilians:

> If Admiral Raborn runs into any difficulty with which I can help, I will want to know about it at once along with his recommended course of action for me to take. If more money is needed, we will get it. If he needs more people, those people will be ordered in. If there is anything that slows this project up beyond the capacity of the Navy Department we will immediately take it to the highest level...[32]

The memorandum also highlights Burke's concern over the urgency of consolidating the idea of an FBM, built and run by the Navy. Believing that 'the first service that demonstrates a capability for this is very likely to continue the project and that others may very well drop out',

30. BuOrd and BuAer were combined in a Bureau of Naval Weapons (BuWeps) in 1958.
31. Quoted in Lockheed Missiles & Space Company, Inc., Fleet Ballistic Missiles - 25 Years (Sunnyvale, Calif.: Lockheed Missiles & Space Company, Inc., n.d.), 1.
32. The memorandum is reprinted in Lockheed Missiles & Space Company, Inc., The Fleet Ballistic Missile System: Polaris, Poseidon, Trident (Sunnyvale, Calif.: LM&SC, n.d.), 6-7.

he called for an early demonstration firing 'even though the equipment in the ships is not as desirable as can be conceived'.[33] In the very first instance, 'technical' feasibility mattered only in so much as it affected the ability of the Navy to retain control of the programme.

Raborn proved to be a felicitous choice for Director of SPO. His personal enthusiasm helped create an atmosphere approaching religious fervour in SPO. A feeling of eliteness was encouraged (unusually for Washington uniforms were required for Naval personnel), and overtime was the norm, as Raborn 'put himself and everyone around him on a wartime footing'.[34] Raborn made it the standard practice in SPO to work Saturday mornings, reportedly joking that:

> We may not get much done here on Saturdays, but by gosh, people are going to know that we're dedicated.[35]

Raborn was also adept at working the bureaucracy towards two interlinked objectives: getting the resources needed quickly to meet an urgent schedule, whilst preventing outside interference in the programme. Taking full advantage of the general urgency over missile developments, especially following Sputnik, Raborn and Burke were able to provide SPO with powerful manifestations of its eliteness. For all but five months (between February and July 1957) the Polaris programme was assigned top priority 'DX' rating which in theory entitled it to priority over 'DO' rated programmes in the allocation of resources.[36] In January 1958 this was supplemented by the creation of a special 'management fund', which, if not in itself greatly adding to SPO's accounting flexibility, provided another powerful symbol of SPO's status.[37]

33. Ibid.

34. Baar and Howard, 43; also Robert E. Hunter, 'Politics and Polaris: The Special Projects Office of the Navy as a Political Phenomenon' (unpublished Senior Honours Thesis, Wesleyan University, June 1962), 61.

35. Ibid, paraphrased from Raborn's regular 'peptalk'.

36. Ibid, 56.

37. Sapolsky, 187-88, refutes beliefs that the management fund greatly enhanced SPO's ability for flexible accounting.

In any case it was prior to Sputnik, especially during 1957, that SPO's direct budget allocation most required supplementing. With Burke's support, however, SPO was able to borrow funds originally allocated to other Navy programmes so as to avoid delays whilst Congress made the necessary appropriations.[38] In addition funds from the canceled Regulus II, Triton and Seamaster (strategic seaplane) were reprogrammed to pay for early Polaris development.[39]

Raborn, with the help of Burke, sold the FBM as a concept and ensured SPO's programmatic independence and access to almost unlimited resources. To do this required skilful manipulation of the social world (the Administration, Congress, the rest of the Navy, etc) on the basis of hypothetical technical projections and assumptions. But for SPO to maintain its pre-eminent position, for the social network they had engineered to remain in place, these promises of technical success would have to be kept. Raborn and Burke had given the programme an ideal start bureaucratically, but the technical people picked by Raborn needed simultaneously to engineer the physical world to produce FBM technology that 'worked'.

Jupiter

At first the newly assembled SPO team had the problem of making the large, liquid-fueled Jupiter IRBM 'work'. Its sheer size and the volatility of its fuel made it seem quite unsuited to submarine launching, and only marginally more attractive for deployment on ships. Whereas the Army's only size limitation was apparently the Berne International Railway Tunnel[40] - to allow 'rapid' transportation around Europe - SPO hoped to carry several onboard submarines. In an attempt to make the missile's shape more suitable for basing on ship or submarine, SPO proposed that Jupiter's envelope be changed from about 90 feet in length and 95 inches in diameter to a 50-foot length, 120-inch diameter missile.

38. Hunter, 100.
39. Ibid, 104; Sapolsky, 35.
40. E. Rees, The Seas and the Subs (New York: Duell, Sloan and Pearce, 1961), 139.

This led to a compromise worked out by Secretary of Defense Wilson for a missile of about 58 feet in length and 105 inches in diameter.[41]

This missile would continue to be developed by the Army's German team in conjunction with their main contractor, Chrysler Corporation. SPO's responsibility was to develop a sea-launching platform, with the necessary fire control and stabilization systems for that purpose. Their schedule was to have a ship-based IRBM system ready for operational evaluation by January 1, 1960, and a submarine-based one by January 1, 1965.[42]

However, right from the start SPO was deeply dissatisfied with the liquid fuel IRBM. Post-war tests of captured German V2s had instilled a deep fear of putting liquid-fueled missiles on Naval vessels. Two other operational problems were also noted, as SPO sought to investigate solid propellant options. Firstly, the cryogenic liquid fuel was not only very dangerous to handle, but also very time-consuming. The time between the firing command and actual launch could be hours, and the missiles could not be kept permanently fueled. Second, an argument was made that liquid-fueled rockets provided relatively low initial acceleration, which could be disadvantageous in launching a missile from a moving platform in certain sea states.[43]

Whatever the merit of these particular arguments, Admiral Raborn raised the issue of SPO investigating solid propellants at the first meeting of the Joint Army Navy Ballistic Missile Committee. This was blocked at the next level of management, the Office of the Secretary of Defense Ballistic Missile Committee (OSDBMC), which saw the Navy request as an attempt to initiate the fifth ballistic missile programme barred only a few months previously.[44] However, the Navy had already taken the initiative

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41. Robert A. Fuhrman, 'The Fleet Ballistic Missile System; Polaris to Trident', Journal of Spacecraft, Vol. 5, No. 5 (Sept-Oct 1978), 265-86, at 267.
42. Ibid, 266.
43. Ibid, 267; see also N. L. Baker, 'Polaris Pioneers Future Ballistic Missile Design', Missiles and Rockets (February 1958), 137, which stresses the advantage of the higher acceleration of solid fuel.
44. See Sapolsky, 25-26.

by approaching the Aerojet-General Corporation and the Lockheed Missile and Space Division for technical assistance in developing a solid-fueled ballistic missile.[45]

This led to a solid-fueled design based on the largest solid propellant motors that could be developed at the time, using one for the second stage and a cluster of six for the first. Designed to carry the proposed Jupiter payload of 3000 pounds to the designated 1500-mile range, this solid design, though shorter than the Jupiter, was both heavier and larger in diameter. To de-emphasize the radical change in technology the new missile was shrewdly called the Jupiter S.

It soon received the backing of the Navy Secretariat and in March the OSDBMC approved the Jupiter S as a 'back-up program' for the IRBM No. 2.[46] The Navy was now officially in the business of developing a solid-fuel ballistic missile. An experienced missile engineer, Captain Levering Smith, who had previously developed the solid-propellant Big Stoop missile at the Naval Ordnance Test Station, joined SPO in April to direct this work.[47]

However, the Jupiter S was considered only marginally more practical a weapon system than the liquid-fueled Jupiter. Just as the original joint missile concept was the Navy's way of 'buying' into ballistic missile work, so the Jupiter S was a way of 'buying' into solid propellant missiles. Both served their purposes, but neither was to come near to fleet deployment. Without even being built they proved to be important links in the development of FBM technology. Whilst work continued both on the joint project and on the Jupiter S, other approaches were intensively pursued.

In particular, Captain Smith requested his former staff at the Naval Ordnance Test Station at China Lake in California to do some system

45. Ibid, 26.
46. Ibid, 27.
47. Baar and Howard, 15. Big Stoop was tested three times in 1951 to a range of about 20 miles.

improvement studies. These suggested that a radical redesign of all the missile's components could make a 30,000 pound missile feasible on the same time schedule as the 160,000 pound Jupiter S.[48]

SPO, however, did not push for the acceptance of this new approach straightaway. Having just won the right to pursue the Jupiter S it was considered too risky to then ask to move to another design apparently requiring even more technological breakthroughs. Instead SPO consolidated its position and the right of the Navy to an FBM. Plans were made to test a liquid-fueled Jupiter on a surface ship in 1958 and to deploy a submarine-based Jupiter S in 1965.[49]

Polaris Conceived

During 1956 technical advances, both actual and predicted, paved the way for the acceptance of a smaller, solid-fueled design. Navy work on solid propellants demonstrated that the addition of aluminium could result in a large increase in specific impulse. Meanwhile MIT's Instrumentation Laboratory kept SPO well informed of its inertial guidance work for the Air Force, which suggested that a lighter guidance system was possible. Then in the summer of 1956 a vital contribution to the FBM programme came from a somewhat unexpected source - a Navy sponsored National Academy of Sciences summer study on anti-submarine warfare. Known as Project NOBSKA (it was held at Nobska Point, Woods Hole, Massachusetts), the study brought together Edward Teller of the Atomic Energy Commission and Lawrence Radiation Laboratory and Frank E. Bothwell of the Naval Ordnance Test Station.

Bothwell had been working on the studies for SPO which suggested the feasibility of a 30,000 pound solid fuel FBM, and he relayed the concept to the study group. The problem was that this required too many technological advances for SPO to take the risk of endorsing, and then possibly failing and losing the right to any ballistic missile. Warhead technology seemed particularly crucial because small changes in the

48. Sapolsky, 28.
49. Ibid.

weight of the warhead had a multiplier effect on the amount of thrust required to reach any given range. Yet at the time there remained a strong adherence in the Armed Services to the notion that a militarily useful weapon required at least a one megaton warhead.[50]

Just before leaving China Lake Bothwell had heard from Dr John Foster who was head of the weapons group at Livermore, that there were good prospects for developing smaller physics packages* than currently available.[51] Then at the NOBSKA summer study Edward Teller made his famous contribution to the FBM programme. Ever the nuclear salesman, he suggested that nuclear-armed torpedoes could be substituted for conventional ones to provide a new anti-submarine weapon. This seemed inconceivable with the current size of nuclear warheads, and Teller was challenged to support his assertion. In doing so he pointed to the *trend* in warhead technology, which indicated reduced weight to yield ratios in each succeeding generation. When asked about the applicability of this to the FBM programme, he asked, 'Why use a 1958 warhead in a 1965 weapon system?'[52]

If correct, Teller's prediction provided just the technical basis on which SPO could push for their preferred option of a small, solid-fueled missile. Already, by mid-July 1956 the Secretary of Defense's Scientific Advisory Committee had recommended that a solid-propellant missile programme be fully instigated, but not using the unsuitable Jupiter payload and guidance system.[53] Official confirmation of Dr Teller's prediction was sought from the AEC, whilst Captain Levering Smith was given a couple of weeks to prepare technical specifications for the small missile he had long supported. When the AEC backed up Teller's estimate in early September Admiral Burke and the Navy Secretariat decided to support SPO in vigorously pushing for the new missile, now named Polaris by Admiral Raborn.

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50. See Herbert York, Race to Oblivion: A Participant's View of the Arms Race (New York: Simon and Schuster, 1970), 90.
51. Interview.
52. William F. Whitmore, 'Military Operations Research - A Personal Retrospective', Operations Research, 9 (March-April 1961), 263.
53. Fuhrman, 267-8
*. That is, nuclear warheads.

In October 1956 a study group comprising key figures from Navy, industry and academic organizations was set up. This considered the various design parameters of the Polaris system, and the trade-offs between different sub-sections.[54] The earlier estimate that a 30,000 pound missile could deliver a suitable warhead over 1500 nautical miles was endorsed. Armed with this optimistic prediction, and with the AEC's official warhead prediction, the Navy now decided to quit the Jupiter programme altogether, and to seek Department of Defense backing for a separate Navy missile.

Raborn briefed Secretary of Defense Wilson on the advantages of the smaller missile, completing his slide show with an estimation of how much it would 'save' by comparison with Jupiter, because fewer, smaller ships would be required for deployment. This 'saving' seemed crucial in convincing Wilson, who told Raborn, 'You've shown me a lot of sexy slides, young man. But that's the sexiest, that half-billion-dollar saving'.[55] On December 8, 1956 Wilson issued the directive that officially started the Polaris programme.

The Navy, Nuclear Strategy and the FBM Programme

The Navy's changing attitude to the FBM system - from indifference to advocacy - also meant a shift in thinking about its role in nuclear strategy. Following World War II the Navy had not as a whole been greatly interested in a major nuclear role. Some Navy officers were trained to operate nuclear bombs and aircraft were adapted to carry these from aircraft carriers,[56] but this was simply considered an extension of the power projection role of the carrier fleet developed during the war.

Despite the important role played by submarines in the war, the surface fleet, and especially aircraft carriers, remained central to the Navy,

54. Ibid, 268.
55. Baar and Howard, 73.
56. By the end of 1949 the carrier Midway was equipped with aircraft able to carry atomic bombs. See Paolucci, 209.

and were the principal avenue by which officers gained promotion. During the late 1940s cutbacks in the defence budget by the Truman administration were the Navy's main concern. Attempts to prevent reductions in the Navy budget allocation led to what was dubbed the 'Admirals Revolt' in 1949.

What was at issue was the general shift in funding towards the newly formed Air Force, and in particular to the Strategic Air Command which had responsibility for long-range nuclear bombardment. The specific focus of the debate was a proposed new Air Force bomber, the B-36, which was to be capable of delivering nuclear bombs to any part of the Soviet Union. However, the Navy's attack on the B-36 did not restrict itself to criticism of the utility of that particular system, but instead went right to the core of the Air Force's nuclear strategy.

The objectives of the early nuclear targeting plans drew heavily on the strategic bombing experience of the war, during which German and Japanese cities suffered immense destruction from Allied bombing. American targeting policy concentrated mainly on damaging Soviet war-supporting capabilities such as production of petroleum, steel and rubber. However, there was little accurate intelligence concerning targets in the Soviet Union, and so up until the end of the 1940s the practical result was that cities were the main targets. After all, Air Force planners noted, 'what was a city besides a collection of industry?'[57]

This trend towards waging war against enemy cities threatened the Navy's traditional military role and looked likely to leave the Air Force as the main beneficiary. Truman's $14.4 billion ceiling on defence spending, announced on May 13 1948, had fallen far short of the amount desired by the Joint Chiefs of Staff to provide a balanced military capability.[58] Instead the USA would have to rely more and more on the ultimate threat of nuclear attack on enemy populations. In an attempt to deflect the budgetary cuts from them, Navy officers argued against the counter-city

57. Quoted in D. A. Rosenberg, 'The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960', International Security, (1983), 3-71, at 15.
58. D. A. Rosenberg, 'American Atomic Strategy and the Hydrogen Bomb Decision', Journal of American History, Vol. 66, No. 1 (1979), 62-87, at 69.

strategy, on both strategic and moral grounds. In one summary of Navy doubts Rear Admiral Daniel V. Gallery, assistant chief of naval operations for guided missiles noted that 'leveling large cities has a tendency to alienate the affections of the inhabitants and does not create an atmosphere of international good will after the war.'[59]

Navy concerns about the efficacy of the counter-city targeting policy were echoed by analysis of the TROJAN war plan. Approved in December 1948 this required an attack on 70 Soviet cities with 133 atomic bombs to be carried out over the duration of thirty days.[60] Such an attack was expected to create 2.7 million mortalities and an additional 4 million casualties.[61] The following year a study of the TROJAN plan, known as the Harmon Report, predicted that even this level of destruction would not by itself 'bring about capitulation, destroy the roots of Communism, or critically weaken the power of Soviet leadership to dominate the people.'[62]

A small group of Navy officers including Gallery and Arleigh Burke proposed an alternative to the Air Force policy of city bombardment:

> They proposed ... that atomic weapons be used primarily against tactical military targets, such as armies, airfields, oil supplies, and submarine pens, which would have to be destroyed to prevent the Soviet Union from taking Western Europe. They argued that scarce budget funds should be spent on conventional tactical air forces and the rebuilding of Western European armies, rather than on expanding capability for an atomic air offensive.[63]

As a corollary to this, they opposed the B-36 bomber, arguing that instead the Navy should build super-carriers to carry aircraft capable of precision bombing of military targets. In October 1949 top Navy officers, including Chief of Naval Operations Denfield, even went so far as to testify to the House Armed Services Committee with their criticisms of counter-city nuclear targeting. The outcome was that several senior Navy officers were

59. Quoted in Ibid, 70.
60. Rosenberg, 'Origins of Overkill', 16.
61. Rosenberg, 'American Atomic Strategy', 73.
62. Quoted in Ibid, 72.
63. Ibid, 74.

relieved of their positions and that the Navy suffered a demoralizing defeat over the B-36 issue. The Air Force, in the form of the Strategic Air Command (SAC), became the primary agent of strategic nuclear warfare, with dominance over bombers to add to its ICBM monopoly.

After the B-36 defeat there were few left in the Navy who wanted further confrontation. Instead during the early 1950s the Navy stuck to the more central concern of protecting its budget share without publicly questioning the nuclear strategy. Navy nuclear forces expanded in line with the Navy's perception of its wartime role, as bombs carried by aircraft were deployed on the surface fleet. Aircraft carriers first achieved a 'rudimentary nuclear capability in 1950-1951', and were considered by the Eisenhower administration to be part of the nation's 'offensive striking power'.[64] These carrier-based nuclear weapons were assigned to 'targets directly or indirectly of naval interest, such as ports, shipbuilding facilities, submarine pens, and naval airfields'.[65] As was the Regulus I cruise missile, which became operational in May 1954.[66]

It was only with the advent of the FBM programme under Admiral Burke that Navy thinking on nuclear strategy under-went a reorientation, and led to another major attack on Air Force strategic orthodoxy. The issue now was not that the Air Force was targeting cities, but that they were targeting so much else besides, including many speculative military targets, and thus justifying huge force levels. In 1956 Admiral Burke argued that money would be better spent on more conventional forces, especially Naval ones, than on more nuclear forces which were already adequate 'to destroy the USSR several times over'.[67]

By this date Air Force policy was for an all-out attack against the whole range of Soviet targets, civil and military - the so-called 'Sunday

64. Rosenberg, 'Origins of Overkill', 51.
65. Ibid.
66. Paolucci, 211. Regulus I was finally retired from active service ten years later in mid 1964.
67. Rosenberg, 'Arleigh Albert Burke', at 282.

punch'.[68] If launched pre-emptively, on warning of imminent hostilities, the Air Force felt that it could thus achieve a decisive blow. Burke and other Navy and Army sceptics argued that SAC's bomber force would soon be vulnerable to the expected Soviet ICBM force and that they therefore could not be sure that they would be able to strike first. Instead they argued that an 'alternative undertaking' should be planned for, providing for the possibility of 'general war initiated under disadvantageous conditions'.[69]

Initially Polaris had been spoken of as though it was simply an extension of the Navy's tactical role, intended to cover the same types of targets as Regulus and the carrier-based aircraft, but at a longer range. The Navy tried to avoid direct competition with the Air Force by *differentiating* the role of the FBM from the strategic mission of the Air Force. Thus for a while they talked about Polaris 'striking targets of naval opportunity', such as submarine pens and port facilities.[70] Indeed, in its early days SPO was particularly careful to reassure not only the Air Force, but also the dominant group within the Navy, those officers committed to aircraft carriers. In a 1957 article, Admiral Raborn described the role of Polaris, stressing subservience to the carrier force:

> Its tactical mission would be to beat down fixed base air and missile defenses to pave the way for carrier strikes aimed at destroying mobile or concealed primary targets.[71]

However, some Navy planners were keen to 'stake out a claim' to a strategic role.[72] The Naval Warfare Analysis Group's first study of the FBM, distributed in January 1957, recommended that 'population or industrial targets should be specified by CNO as the target for the initial FBM capability'.[73] Polaris now was acknowledged as a strategic weapon,

68. See D. A. Rosenberg, '"A Smoking Radiating Ruin at the End of Two Hours": Documents on American Plans for Nuclear War with the Soviet Union, 1945-1955', International Security, Vol. 7 (1983), 3-38.
69. Rosenberg, 'Origins of Overkill', 53.
70. Sapolsky, 44; see also Baar and Howard, 26.
71. 'Navy Views Polaris as Support Weapon', Aviation Week, Vol. 66 (June 17, 1957), 31.
72. Vice Admiral R.E. Libby quoted in Rosenberg, 'Origins of Overkill', 52.
73. Naval Warfare Analysis Group Study No. 1, 'Introduction of the Fleet Ballistic Missile into Service' (January 1957), Serial 007P93. I am grateful to the author John Coyle for

but was still carefully differentiated from Air Force systems. Skillfully combining national concern over SAC vulnerability with the Navy's dislike of Air Force 'overkill', Burke outlined a strategic concept, now known as 'finite deterrence', whereby retaliation would be threatened by a relatively small, invulnerable force with, as he put it, its size determined by 'an objective of generous *adequacy for deterrence alone* (ie, for an ability to destroy major urban areas), not by the false goal of adequacy for "winning"'.[74]

Such an invulnerable counter-city weapon was, of course, exactly what Polaris was expected to provide. It was to be a strategic weapon, but not (at least not primarily) a counterforce one - its mission was to destroy cities in retaliation for a Soviet strike. This rationale made sense both *vis a vis* the Soviet Union and *vis a vis* the Air Force. Both the pressure for early availability and the evolution of the 'assured destruction' rationale tended to de-emphasize the pursuit of accuracy. Polaris had to be accurate enough reliably to destroy cities - but that did not mean very accurate. If it was less accurate than the Air Force ICBMs, this did not matter much - indeed it could even be taken as an advantage, as a clear technical manifestation of the bureaucratic strategy of 'differentiation'. What mattered most of all though, both in the 'cold war' with the Soviet Union, and in the interservice war with the Air Force, was to get Polaris built as soon as possible, and to show that it 'worked'.

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supplying me with a copy of this report which was declassified in March 1980 by the Office of the Chief of Naval Operations.

74. Rosenberg, 'Origins of Overkill', 56-57.

Chapter 3

Building Polaris

Our religion was to build Polaris.
Admiral Raborn.[1]

Thus by the beginning of 1957 a combination of technological advances (or predictions of advances) and astute political manoeuvring had created the Polaris programme. As director of SPO, Admiral Raborn continued to see his role as managing the outside world, to: 'Get the money, and keep other people off our program managers' backs'.[2] In his now classic study, Harvey Sapolsky notes four strategies which contributed to SPO's bureaucratic success in doing this: *differentiation* of a special role, which was represented as of crucial national importance; *co-optation* of potential critics and disruptive elements; *moderation* of short-term goals in order to maximize long-term support; and *managerial innovation* in order to create an aura of efficiency.[3]

Differentiation meant not only encouraging a feeling of eliteness amongst those who worked on Polaris, but also creating a distinct mission for the FBM force to justify its existence alongside the missile programmes of the other services.[4] Co-optation involved drawing potential critics into becoming involved with and so committed to the programme. Thus money was always available to fund someone who had ideas relevant to Polaris, and good relations with scientists were particularly encouraged.[5] Likewise in an attempt to placate the concern of the surface Navy, SPO frequently raised the possibility of basing Polaris on surface ships during the late 1950s, though only one such proposal was ever given the go-ahead. This plan to deploy Polaris on the nuclear-powered cruiser *Long*

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1. Harvey M. Sapolsky, The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972), 158.
2. Rear Admiral William F. Raborn, 'Navy within a Navy' in 'Rocketry in the 50's', Astronautics and Aeronautics Vol. 10, No. 10 (October 1972), 63-65, at 64.
3. Sapolsky, 41-60.
4. The differentiation of nuclear strategy is discussed in detail in chapter 2 above.
5. Sapolsky, 48-50.

Beach was instituted in January 1961 by the Eisenhower administration, but canceled two months later under Kennedy.[6]

Moreover, whilst SPO sought autonomy over anything affecting the development of Polaris, it was careful to show restraint on issues that were not vital to this concern and that might foster long-term resentment. Such moderation meant, for example, that SPO avoided publicly reciprocating the Air Force's vocal criticism of Polaris. Finally, SPO had a new management system devised, the Program Evaluation Review Technique, known as PERT.[7] This was initially very unpopular with SPO's technical staff and with the contractors, but it soon came to be valued for the image it created: 'It had lots of pizzazz and that's valuable in selling a program.'[8] The image of managerial efficiency thus created greatly aided SPO in their job, as Sapolsky has noted:

> An alchemous combination of whirling computers, brightly colored charts, and fast-talking public relations officers gave the Special Projects Office a truly effective management system. It mattered not whether parts of the system functioned or even existed. It mattered only that certain people for a period of time believed that they did.[9]

But the success of the FBM programme did not depend on SPO's bureaucratic prowess alone. Successful heterogeneous engineering certainly requires skilful manipulation of the social world, but it cannot neglect the physical world.

Technical Decisions

A Polaris Steering Task Group (STG), headed by Captain Levering Smith, was set up to oversee the technical development of the programme. This convened for the first time on January 7, 1957. The next few months were then spent defining the technical specifications of the Polaris system, even though 'the exact characteristics of the re-entry

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6. Robert E. Hunter, 'Politics and Polaris: The Special Projects Office of the Navy as a Political Phenomenon' (unpublished Senior Honours Thesis, Wesleyan University, June 1962), 102-4.
7. PERT is discussed in great detail in Sapolsky, 94-130.
8. Quoted in Ibid, 124
9. Ibid, 129.

vehicle and payload were still an educated guess'.[10] An initial, fundamental issue was to decide on the performance goal of the system. Delivery of a one megaton warhead to a range of 1500 miles was the original goal set for the IRBMs by the Killian Report. This remained SPO's eventual goal, but was relegated to a later development. In the meantime an interim Polaris missile was to be rapidly built, providing the nation with deterrent capability at the earliest possible time, and more pertinently, consolidating the Navy's claim to a ballistic missile. SPO's Chief Scientist recalls the flexible way they viewed the performance goal of the first Polaris:

> So it was clear that we were not confronted with a standard request to design an optimum system meeting a fixed operational requirement. The task was rather to set minimum acceptable initial operational performance, expecting the total system capability to improve with time ... In very crude terms, we felt that the Polaris missile had to be able to reach Moscow from a position at sea and cause a reasonable amount of damage when it got there. Based on our knowledge of geography and weapon effects in 1957, this implied about 900 miles range and a half-megaton yield.[11]

The FBM System operational requirement specified in February 1957 was: 'provide an all-weather capability to deliver from ships to strategic land targets at intermediate ranges, with minimum susceptibility to countermeasures, a weapon which will provide the required damage probability.'[12] In May 1957 SPO redefined their schedule, calling for an interim Polaris A missile, with nominal range of 1200 nautical miles, to provide a surface launch submarine capability by January 1, 1963. Full submerged launch and a 1500-mile range were to be provided with the Polaris B by January 1, 1965.[13]

The programme was then accelerated further following the Soviet launch of the Sputnik satellite on October 4, 1957 and the

10. William F. Whitmore, 'The Origins of Polaris', United States Naval Institute Proceedings Vol. 106 (March 1980), 56-59, at 57.
11. Ibid.
12. Robert A. Fuhrman, 'The Fleet Ballistic Missile System; Polaris to Trident', Journal of Spacecraft, Vol. 5, No. 5 (Sept-Oct 1978), 268.
13. Ibid.

recommendations of the Gaither Report in early November.[14] Deployment of the interim missile, now known as Polaris A1, was rescheduled first to 1961, and then to November 1960. Deployment of A1X test missiles to provide an even 'more interim' capability - of about 1000-mile range - somewhat earlier was also considered, if emergency measures were invoked.[15]

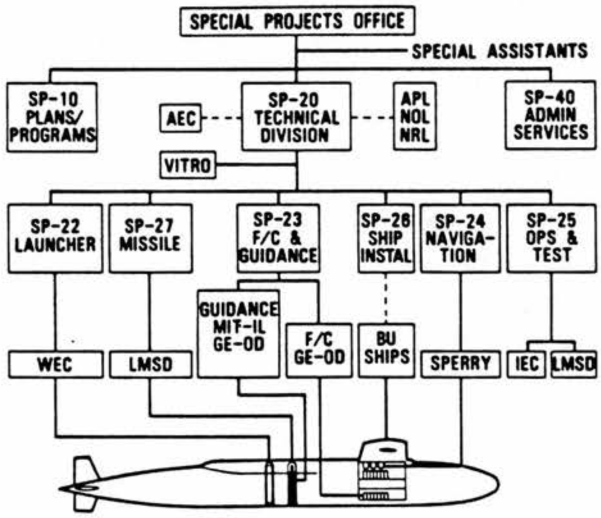
To meet this schedule SPO decided to use the hull of an existing nuclear-powered submarine currently under construction at Electric Boat's shipyard in Groton, Connecticut. This was to be literally cut in half and a missile section inserted. The length of this section was determined by the number of missiles each submarine was to carry and the method of stowage. The preferred stowage configuration was vertical, two abreast. There was less consensus about the optimum number of missiles per submarine. Economic considerations pushed towards large numbers, up to 32 per vessel, whereas operational flexibility, survivability, and the preferences of submarine commanders pushed the other way. The question defied exact analysis and in the end was put to 'a sort of opinion poll of ship designers, analysts, and submarine operators'.[16] This looked likely to result in a twenty-four tube design, but sixteen was chosen by the intervention of Admiral Raborn - as always sensitive to the need to enrol support - once he learnt that this was the maximum that the submariners felt desirable.[17]

Submarine conversion was estimated to take about four years and thus needed to begin without delay. Missile development was not expected to take as long, perhaps only two years, but obviously the roughly simultaneous development of the various elements of the system had to be compatible when brought together. It was thus critical right at the start to define the subsystem 'envelopes' and their interfaces, something which the STG did during the first few months of 1957.

14. D. A. Rosenberg, 'Arleigh Albert Burke' in R. W. Love, Jr., The Chiefs of Naval Operations (Annapolis, Maryland: Naval Institute Press, 1980), 284-85. The Gaither Report was officially known as the report of the Security Resources Panel of the Science Advisory Committee.
15. Fuhrman, 268.
16. Whitmore, 58.
17. Sapolsky, 54.

Each subsystem was assigned to a separate technical branch of SPO, most of which had already been formed for the sea-basing of the Jupiter. SP-22 was responsible for the launcher subsystem, which needed to be able to store the missile for long periods of time, and ideally allow submerged launch. SP-23 had handled fire control for the Jupiter and now took on missile guidance as well (which was initially intended to be assigned to the missile branch). The two were grouped together because of the difficulty of clearly defining their interface at this early stage.[18] SP-24 was navigation, critical to the overall accuracy of the system because of the importance of knowledge of the initial launch position. SP-26 was ship installation, devoted to the building of the submarines and installing the other subsystems. Finally, with the start of the Polaris programme another branch was formed, SP-27, for the missile subsystem, including warhead and re-entry vehicle but not guidance.

Each branch chief would have responsibility for their subsystem, reporting regularly to Levering Smith, who became Technical Director of SPO in June 1957, and to the Steering Task Group which met every few months. (See Figure 3.1 for Organizational Chart of SPO and its contractors.)



Special Projects Office Organization - 1957

Figure 3.1: SPO Organization - 1957.

Source: Fuhrman, 270

18. Interview.

Launcher Subsystem

By the time Polaris was approved, SPO's launcher branch, SP-22, headed by Commander Dennet 'Deke' Ela had already spent a lot of time thinking about the problem of launching the Jupiter missile from a surface ship. A major concern with the liquid-fueled Jupiter, of course, was safety, and launching from the rolling, pitching, windswept deck of a ship exacerbated this concern. It was decided that the dangers involved in fueling the missile would be minimized if done below decks, with the missile then raised immediately prior to launch. This would require an elevator system, but that was a technology much used by the Navy for moving planes around in aircraft carriers.

This scheme was approved and Westinghouse Electric Corporation's elevator division were contracted to build it. The plan was to install three missiles per ship, starting initially by converting a Mariner-class merchant vessel, the *Observation Island*.[19] Submarine launching for Jupiter seemed a much longer term prospect because no current nuclear-powered submarine design was anywhere near large enough to accommodate the missile. Although large by the standards of the day, the world's first nuclear powered submarine, the *Nautilus* had a displacement less than half that considered necessary to carry four Jupiter missiles.[20] And although slightly shorter the solid-fueled Jupiter S was both fatter and heavier.

With the arrival of the concept of Polaris, size was no longer a problem, and the sixteen tube submarine was decided upon. Surface launch also seemed relatively simple to achieve, but the militarily preferable submerged launch did not: 'The means to do this were simply not identified and the capability to do it was completely unknown'.[21] Westinghouse were retained as the launcher contractor, and they established a systems group to tackle the problem. Two options were

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19. James Baar and William E. Howard, Polaris! (New York: Harcourt, Brace & World, 1960), 66.
20. Ibid.
21. Interview.

identified. The one considered the more conservative involved releasing the missiles from the submarine in capsules which would then float to the surface and open up to allow launch. The other was bare missile launch, where the missile would be launched unprotected through the water.

The two approaches were pursued simultaneously for some time on the basis of a missile size, shape and weight 'envelope' defined by the Steering Task Group. However, although size and shape were by now defined quite precisely, weight and structural load were not. The launcher branch was thus in the difficult position of having to meet an urgent schedule whilst retaining some flexibility:

> The missile was the least well defined of all these things in this timescale and so the people who were designing the launcher were caught between the ship-builder who was actually making things and the missile people who were still trying to define what this missile was like in terms of its structural load, and so on... So the job of designing the launcher got to be to figure out how you could build something that was sufficiently flexible in its design that you could define the interfaces it had with the ship under circumstances when the interfaces it had with the missile were only vaguely defined. ... You didn't know what the shock resistance of the missile was going to be. You simply had to have enough rattle space that whatever it turned out to be you could accommodate that.[22]

This 'over-designing' of the launcher system would later prove highly significant, allowing larger missiles to be developed for use within the same submarines.

In June 1957 Captain Ela of SP-22 established a deadline for the first underwater launch of a dummy missile - by January 1958.[23] Scale model testing done at the Naval Ordnance Test Station suggested that the bare missile could be launched at a depth of the order of a hundred feet and still arrive at the surface sufficiently vertical to recover its trajectory when ignited.[24] The launcher subsystem was required to provide 'a protective cocoon that would cradle the missile against both lateral and vertical

22. Ibid.
23. Baar and Howard, 111.
24. Interview.

shock', eject it with sufficient velocity when required, and withstand the effect of the backflooding seawater.[25]

It was decided to hold the missile snug within the launch tube with three rings of flexible pads, known as stowage launch adaptors, which would fall off when the missile was ejected. The nature of these pads was considered to require a very rigid, smooth launch tube for which heavy-walled, machined steel was chosen. This launch tube was to be suspended within the submarine mount tube, with a requirement that it be able to withstand the shock of an under-water depth charge explosion. Oil-filled double-acting Dowdy shock absorbers were chosen to provide this suspension as they could be made with a 'null' position from which they deviated only after experiencing a significant force. The advantage this had over competing spring technologies was that it facilitated the precise positioning of the missile for the optical alignment required by fire control to 'ready' the missile guidance system.[26]

Missile ejection was achieved by compressed air pressure controlled by a programmed air valve adapted from the type used in catapult equipment for assisting plane take-off from air craft carriers. Prior to launch the heavy outside hatches covering the launch tubes would be opened leaving the tube sealed only by a thin diaphragm which was explosively removed at the instant of launch.

Thus the main elements of the launch system evolved.[27] At each stage testing was carried out, with the scale models replaced first by redwood logs launched from the 'Peashooter' test facility at the San Francisco Naval Shipyard, and then by inert test vehicles supplied by the missile contractor, Lockheed. Underwater testing was performed at the 'Pop-up' test site of the Naval Ordnance Test Station's underwater test range near San Clemente Island. The first underwater test was carried out there in March 1958, after a delay due to bad weather, when a dummy missile was successfully 'launched'.

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25. Westinghouse, To the End of the Rainbow - Evolution of the Marine Division (typescript), 49. I am grateful to David Nixon of Westinghouse for providing me with this.
26. Interview.
27. See 'Polaris Launcher Production Hits Four a Week', Missiles and Rockets, Vol. 10 (February 12, 1962), 32-33.

Guidance and Fire Control

SP-23 was initially set up to develop a fire control system to coordinate the Jupiter guidance system, designed by the Army's German team, with the ship or submarine navigation system, and to perform all the functions necessary for launching at the desired target. When the switch was made to Polaris the Navy became free to choose its own guidance team rather than relying on the Army's, whose approach, although feasible, became organizationally unappealing. The German team had worked with Bendix on a smaller guidance system which might have been suitable for Polaris, but their enthusiasm was not matched by that of the head of the Army's Ballistic Missile Office, General Medaris. One SPO officer recalls that Medaris 'was not very fond of the Navy from the beginning and he and Raborn didn't get along worth a dime'.[28]

But SPO's urgent schedule, as well as their service pride, made relying on the Army's expertise an uninviting option, if it could be avoided. It could, as indeed SPO had become increasingly aware of prior to the break with the Army. An alternative was to be found at the MIT Instrumentation Laboratory with which SPO had contracted to investigate ship stabilization for the Jupiter missile, and whose pioneering work on inertial navigation was also to feed back into the Polaris submarine navigation system.

Under the directorship of Charles Stark Draper the Instrumentation Laboratory had been at the forefront of developing inertial technology for navigation and guidance since 1945. Refining a gyroscope design developed during the war for gun control systems, Draper became an influential proponent of inertial technology.[29] Whilst working with SPO on ship stabilization for the Jupiter, the Instrumentation Laboratory was also developing a guidance system for the Air Force's Thor IRBM. One member of the team working on the Air Force contract remembers:

28. Interview.

29. See D. MacKenzie, 'Missile Accuracy: A Case Study in the Social Processes of Technological Change', in W. E. Bijker, T. P. Hughes and T. Pinch, The Social Construction of Technological Systems (Cambridge, Mass.: MIT Press, 1987), 195-222.

> ...it looked twenty years ... before they [would] ever put a ballistic missile in a submarine. Submarines did not want [liquid] ballistic rockets, and ... the [Jupiter] missile was just plain ... too big and too unsafe for the submarines to put up with ... I said to Sam Forter [Commander Forter, an MIT alumnus, in SP-23] ... 'What we really ought to be doing for you is ... doing studies on a smaller ... ballistic missile for a submarine ... we know how to make you a small one' ... So he took me in to talk with then Captain Smith [SPO's Technical Director] ...[30]

In addition, SP-23's branch engineer Dave Gold had worked with the Instrumentation Laboratory as the Bureau of Ordnance's project engineer on Project MAST, an early application of inertial technology to weapon system stabilization.[31] SPO arranged a contract with the Instrumentation Laboratory using Bureau of Ordnance funds ('the Navy didn't care too much as long as we didn't embezzle it'[32]). So the Instrumentation Laboratory 'had six months head start on a design for a ballistic missile for submarines before the Polaris program was signed and given a name'.[33]

As well as their general inertial expertise and their familiarity to many in the Navy, the Instrumentation Laboratory also could offer another crucial 'technical detail': a mathematical guidance formulation which seemed particularly well-suited to Polaris. Moving to a smaller missile placed a high premium on miniaturization, which was clearly a constraint on the onboard computer that had to perform the computations necessary to guide the missile. Digital computation seemed to be taking over from analogue, but by modern standards was slow and bulky. Two mathematicians at the Laboratory, Richard H. Battin and J. Halcombe Laning Jr., through work they were doing for the Air Force Atlas ICBM, developed a mathematical scheme that became known as 'Q-guidance'. The enormous advantage of this method was that, though a lot of computation was involved, much of it could be done well in advance of

30. Interview.
31. See W. F. Raborn and J. P. Craven, 'The Significance of Draper's Work in the Development of Naval Weapons', in S. Lees (ed.), Air, Space and Instruments: Draper Anniversary Volume (New York: McGraw-Hill, 1963), 23.
32. Interview.
33. Ibid.

firing the missile (the calculation of the elements of the Q matrix), leaving only fairly simple tasks for the onboard computer.[34]

Whilst still working on the liquid-fueled Jupiter, SPO was discussing Q-guidance with people at the Instrumentation Laboratory. On one occasion Ralph Ragan and David Hoag of the Instrumentation Laboratory and Sam Forter of SP-23 tried to explain the principle of Q-guidance to SP's Technical Director, Levering Smith. When they became confused over how it really worked, it was Levering Smith who eventually clarified the principle.[35] He became convinced that it was particularly suitable for guiding solid-fueled missiles 'because Q-guidance did not need to adjust the thrust program in flight as others did. Unlike liquid-fueled missiles there was no practical means for adjusting the thrust program of solid-fueled missiles'.[36] Admiral Levering Smith recalls that MIT was chosen over the Redstone Arsenal at Huntsville 'primarily because of the Q-guidance. It did appear that we could work more closely with Draper than with Huntsville, partly because I thought the [Draper] fluid floated gyro would adapt easier to the solid motor accelerations, but to my way of thinking it was driven more by Q-guidance than anything else'.[37]

The culmination of all this was that on 10 October 1956 Raborn and some of his staff 'visited Draper to elicit his interest in developing the Polaris inertial guidance. The result was a direct contract for its development... The General Electric Company was selected to provide industrial support and to build the resulting guidance system'.[38] Thus was set the organizational pattern that has persisted to this day for the development of Fleet Ballistic Missile guidance systems: a direct contract awarded to the Draper Laboratory, with the systems designed by Draper being produced by major industrial firms.

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34. Interviews; see also R. H. Battin, 'Space Guidance Evolution - A Personal Narrative', Journal of Guidance and Control, Vol. 5 (1982), 97-110.
35. Interview.
36. Letter from Vice Admiral Levering Smith to Donald MacKenzie (13 October 1986).
37. Interview.
38. Raborn and Craven, 27. General Electric had already been working on a radio-inertial guidance scheme for Jupiter.

Working on a very tight schedule, which became even tighter in December 1957 following the launch of Sputnik, the Instrumentation Laboratory designed and developed the Mk1 guidance system for Polaris. The gyroscope was a 'paradigmatic' Draper design: the 25-size (i.e. 2.5 inch diameter) inertial rate-integrating gyroscope (IRIG), which had been developed for the Air Force. The accelerometer was a pendulous integrating gyro accelerometer (PIGA) based on the same 25-size gyroscope. 'Each PIGA contained a [gyroscope] which was the same design as the 25-size IRIG with an additional unbalanced mass'.[39] Three 25-IRIGs mutually at right angles held a stable platform carrying three similarly positioned 25-PIGAs in a known orientation. This platform was supported by three gimbals made of beryllium allowing relatively free, but not unlimited, movement. The onboard computer was the first digital fully transistorized guidance computer, but it was not a full general-purpose digital computer. Of the type known as a Digital Differential Analyzer, it was customized to perform the few repetitive calculations required to solve the differential equations used in Q-guidance. Using germanium components - discrete diodes and transistors - the Mk1 electronics had a total gate count of about 400, comparable to a modern digital wrist-watch.[40]

The Mk1 guidance system combined inertial components and electronics into one module weighing 225 pounds.[41] Relatively simple in concept, the Mk1 was a nightmare to make, with difficulties in 'every aspect of production'.[42] With the computer there was 'a lot of trouble with the memory cores...trouble in the wiring and testing...and there was trouble with the transistors' - 'they had a terrible time getting any degree of reliability out of it.'[43] Participants still remember the 'purple plague': 'at the junction of the lead to the transistor they would start to rot, mould, or whatever you call it, and under the right light, or maybe by bare eye, it turned purple, and you lost connection...it was like a disease that went through all the early transistors'.[44] Gyroscopes also were very difficult to produce to the standard required: 'You'd make a batch of bearings that

[39]. B. O. Olson, 'History of FBM Guidance at CSDL' (10 March 1975, typescript), 2.
[40]. Interview.
[41]. Olson, 2.
[42]. Interview.
[43]. Ibid.
[44]. Interview.

would work phenomenally...and had life times of 100,000 hours. And then a year later all the production people... were getting poor gyros. All the bearings would go bad in all the production lines.'[45]

Nevertheless, the Mk1 guidance system was ready in time to meet Polaris schedules. Although its mean time between failures was not ideal, with careful attention it was adequate for the short duration of guided flight - of the order of a minute.[46] During first stage flight the missile's flight was to be controlled by a programmed autopilot, with the guidance system controlling second stage flight up to thrust termination and separation of the re-entry vehicle.

Meanwhile SP-23 was also working on a fire-control system to prepare the missile guidance systems for launch. Information from the navigation system was communicated to missile guidance systems through the fire control system. The original Polaris fire control system, the Mk. 80, was 'less of a system than it was just what could be done in the time'. It had little computational capability: it was more a 'card reader'.[47] Crucial data such as the components of the Q-matrix were calculated onshore, at the Naval Ordnance Station in Dahlgren, Virginia, and the data for a particular target read into the fire control system on cards. After the first ten submarines, and for the UK Polaris fleet, it was replaced by the Mk. 84 system. This did have a digital computer integral to the system - 'a "militarized" version of Control Data Corp's "1604" commercial computer' - which provided a more rapid on-board retargeting capability.[48]

Fire control not only provided the missile guidance systems with navigation information and targeting data, but it also played a role in preparing them physically for launching: the processes known as 'alignment' (determining orientation in the horizontal plane) and 'erection' (determining the direction of the vertical). 'The guidance interface was quite complex in that all the loops... were closed through fire

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45. Ibid.
46. R. B. Walter, 'The "Brain" of the Polaris Missile', Missiles and Rockets, Vol. 8 (June 12, 1961), 30-31, 53, at 30.
47. Interview.
48. M. Getler, 'Improved Polaris Fire-Control System Going to Sea Duty Shortly', Missiles and Rockets, Vol. 13 (November 4, 1963), 32-33, at 32.

control, so that alignment and erection was done through fire control... All of that interface..., connected through resolvers, was extraordinarily complex, very touchy. Everything had to be done just right for both alignment and erection. Quite honestly I look back on it and it's a miracle it ever worked, but it did, and does'.[49]

Navigation

However, the missile guidance system was initially perceived as less demanding than the closely related question of submarine navigation. An error in knowledge of the submarine's position at launch would lead to missing the target, and even a small error in azimuth (orientation in the horizontal plane) could lead to a very large miss. State-of-the-art submerged submarine navigation was approximate at best, and the capacity of the nuclear submarine to remain submerged for long periods would be of no value if frequent surfacing was required to work out position and azimuth.

A possible solution existed, but in 1956 had not been proven for submarine use. Self-contained inertial navigation had been developed at the MIT Instrumentation Laboratory, under Draper, and at the Autonetics Division of North American Aviation, for use in bomber aircraft and long-range cruise missiles. The Instrumentation Laboratory had also worked for the Navy on applying this technology to submarines, even before the FBM was conceived. In 1951 the Laboratory was awarded a Navy Bureau of Ships contract to develop a prototype Ships Inertial Navigation System (SINS), which they delivered in 1954. For the Polaris programme, the Laboratory's work was taken up by the Sperry Corporation, pioneers of gyroscope technology, and a firm with especially strong links to the US Navy.[50]

A 'little detail' of the MIT/Sperry design led to almost as much anxiety for the developers of Polaris as any other issue. Charles Stark Draper of MIT believed in the elegant virtues of letting gyroscopes remain in fixed orientation in inertial space (ie with respect to the stars). But as

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49. Interview.
50. Raborn and Craven, 25.

the earth rotated and the submarine's position changed this meant that the gyroscopes were subjected to a varying gravity field. The slightest mass imbalance of their rotors would lead to significant errors. But achieving perfect or near perfect mass balance was an exceedingly difficult task, especially as one moved outside the laboratory to the 'real world' of production.

Fortunately for Polaris's tight schedule an alternative was available. As interest in ballistic missiles grew during the 1950s, air-breathing cruise missiles fell out of favour. In July 1957 the 5000-mile US Air Force Navaho was abruptly canceled and the Autonetics Division of North American Aviation were left with a guidance system known as the Autonavigator. Autonetics moved quickly to find a new customer, and received a contract from SPO to adapt the Autonavigator for submarine use. The XN6 guidance system developed for Navaho incorporated a novel feature - each of the three axes had two gyroscopes which could be reversed, averaging out 'drift' of the gyros. Unlike the MIT/Sperry design, it was a 'local level' system, kept horizontal at all times, so the gyros were not subject to sudden, large changes in the direction of gravity. By 1958, the XN6 system, modified as the N6A Inertial Navigator, was mature and reliable enough to be taken on a submarine mission that ensured its fame and rescued it from the status of a component in a canceled system. It navigated the USS *Nautilus* on its widely publicized voyage from the Pacific to the Atlantic under the ice surrounding the North Pole.

SPO ran sea trials on their test platform, the USS *Compass Island*, to compare the performance of the Sperry/MIT SINS, known as the Mk-1 and the N6A. The Autonetics SINS performed much better, but both companies were awarded contracts to develop a SINS for Polaris. With navigation requirements so stringent by the standards of the day, Sperry supporters found it easy to argue for duplication; Admiral Raborn himself particularly favoured maintaining competition in the navigation system.[51]

[51]. Interview.

At this point a program initiated in 1954 by the Underseas Warfare Branch of the Office of Naval Research came to fruition.[52] This led to the development of gyroscopes in which the the ball-bearings of the early designs were replaced with gas spin bearings with a substantial improvement in performance. A new SINS using the gas spin gyro was developed by Autonetics.

Meanwhile, it seemed the modified Sperry system, the Mk3 Gyronavigator, would be installed in the first five submarines, the 598-class. The new Autonetics design, the Mk2 Autonavigator, was to go in the next five 608-class. However, when it came to the decision the Autonetics SINS was 'much nearer to being ready than the Sperry' and this deployment pattern was reversed.[53] After a few years these Sperry systems were replaced and the Autonetics Mk2 (in various modifications) became standard on FBM submarines.[54]

The Mk2 SINS was based on a stable platform carrying three orthogonal G7A self-activating gas spin gyroscopes which sense accelerations and maintain the known orientation of the platform via servo motors. Three orthogonal accelerometers then measure the accelerations which the platform experiences. By 'deducting' gravitational accelerations, it is possible to derive velocity and position by integration.

But even the best SINS cannot operate autonomously for an indefinite time. Periodic 'resets' - updates from external sources of navigational information - are needed to stop unacceptable errors building up. In Polaris it was decided to do this every eight hours if possible.[55] In addition, the SINS, like any inertial system, needs knowledge of the

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52 See B. McKelvie and H. Galt, Jr. 'The Evolution of the Ship's Inertial Navigation System for the Fleet Ballistic Missile Program', Navigation: Journal of the Institute of Navigation, Vol. 25 (Fall 1978), 310-322, at 312.

53. Interviews.

54. A considerable mystique developed around the admittedly demanding SINS technology. Extraordinary restrictions, for example,were placed on British access to the SINS that were sold to the UK as part of the Polaris purchase. The UK, including the captains and navigators of the Polaris submarines and the Admiralty's technical specialists, were not and are not allowed navigational data from the SINS. It is transmitted directly to the American-provided fire-control system and thence to the missile, with its diversion into British (human) hands prohibited.

55. Interview.

gravitational field through which it is passing. These necessities implied heterogeneous engineering of a potentially difficult kind, since delicate matters of international relations were involved.

One solution to the reset problem free of international relations difficulties was to use the human navigator's old stand-by, the stars. The 'type 11 periscope' enabled 'an operator to visually locate a true star position and manually enter the information which corrects the inertial system's prediction of the ship's position'. In use the 'Type 11 was a real dog. I mean it was a mechanical marvel, but it was a hydraulically-driven, hydraulically-supported periscope: it's like taking... sights at the top of a 40 foot pole, and you've got to remember that you've got to track and everything else while the ship is moving all over the place, and we were only too happy to get rid of it'.[56] With its 'usefulness...limited by marginal accuracy, cloud cover, daylight, alignment problems, and maintenance costs'[57], the Type 11 had been discarded by the time the navigation systems for the British Polaris submarines were supplied by the US, and was finally eliminated from the US fleet in 1969.[58] A similar technique provided resets from radio wave emission from the sun or moon using a radiometric sextant, providing an all-weather capability lacking in the Type 11.[59]

However, three other sources of external fixes were concentrated on.[60] One involved surveying the sea floor with sonar and identifying distinctive features. The first Polaris submarines were to be deployed in the Norwegian Sea, and so initially only a limited area needed to be surveyed. The submarine could then navigate from one surveyed feature to another, updating its SINS at each, in just the same way that modern

56. Interview.

57. S. A. Conigliaro, 'From Polaris to Trident Navigation' (Mimeo of speech given to National Marine Meeting, Institute of Navigation, US Merchant Marine Academy, Kings Point, Long Island, New York, 23 October 1973), 6; 'marginal accuracy' had been added to the text by hand, in the copy provided to me at Unisys.

58. Interestingly, however, periscopes of this general kind remain in use in the French submarine-launched ballistic missile fleet, whose dependence on other, American dominated, navigation aids would be unacceptable. Vol. 72

59. See B. Miller, 'Radio Sextant Developed for Submarines', Aviation Week (February 29, 1960), 81.

60. A useful summary of navigation resets can be found in Owen Wilkes and Nils Petter Gleditsch, Loran-C and Omega: A study of the military importance of radio navigation aids (Oslo: Norwegian University Press, 1987).

cruise missile guidance uses terrain mapping. So long as no extraordinary manoeuvres were required, a submarine could follow surveyed features without coming near the surface (except for communications). Of course, both this and accurate gravitational mapping required detailed surveying of future Polaris patrol areas by surface ships. This attracted the attention of the Soviet Union, and Soviet vessels began to shadow the survey ships. Even so the true purpose of the survey ships was apparently considered too sensitive to be imparted to America's NATO allies except in most general terms.[61]

Two further solutions to the reset problem were also pursued. In addition, both helped the survey ships in the task of locating the sea-floor features they mapped. One was a more accurate version of Loran (Long Range Aid to Navigation) known as Loran-C.[62] Loran was developed during the Second World War at the MIT Radiation Laboratory. Time differences between the arrival of radio signals from widely spaced land-based transmitters enabled positional fixes to be made. By the late 1950s Loran-C receivers were able to provide absolute navigational accuracy of about a quarter of a mile at a thousand mile range, and were sensitive to differences of thirty to forty feet.[63] Loran-C was actually an Air Force development which was avidly taken up by SPO's navigation branch. It was used first on the USS *Compass Island*, then for the survey ships as they mapped sea-bed features, and was available for the first Polaris submarines when they went on patrol. By 1962 Loran-C networks were operating in the Northern and Western Atlantic, the Mediterranean, round Hawaii, and in the North Pacific and Aleutians.[64] In these areas a trailing wire antenna would be 'often deployed for continuous reception' of Loran-C so that it could 'be used continuously to monitor SINS performance'.[65]

The new Loran-C stations were built - in Norway, Italy, Spain, Turkey, Denmark, Libya, and elsewhere - without creating political

61. See Ibid, 81.
62. See P. J. Klass, 'Computer Simplifies Loran-C Navigation', Aviation Week and Space Technology Vol. 80 (15 June 1964), 95-97.
63. Interview.
64. D. A. Anderton, 'Loran-C Extension Proposed for Tracking', Aviation Week and Space Technology Vol. 77 (October 1, 1962), 41-47.
65. Conigliaro, 6.

controversy. In future years, however, stations designed - or believed to be designed - for FBM navigation were to lead to open political dispute in New Zealand, Australia and Norway.[66]

Free from such risks, and also at least in the immediate future safe from possible Soviet attack, was the third reset system. This was the world's first satellite navigation system, Transit. Scientists at Johns Hopkins University Applied Physics Laboratory were developing a technique for tracking the orbit of Sputnik using the Doppler shift in the frequency of its transmission at a ground station of known location. By reversing the process a navigational fix could be obtained from a satellite of known orbit.[67] Although not available for the first Polaris patrols, Transit would soon prove to be not only an additional source of navigational fixes, but perhaps more importantly, a major source of geodetic information.[68]

So the standard navigation equipment of the first Polaris submarines was three SINS; an electromagnetic log which measured water speed (necessary to damp oscillations in the SINS); Loran-C receiver, Transit receiver (when developed), Type 11 periscope, and terrain matching sonar (all for updating the SINS); and two NAVDAC (Navigation Data Assimilation Computer) systems to integrate all the information.

Missile

SPO's missile branch, SP-27, was formed when the all-clear was given to develop Polaris in December 1956. A year earlier Lockheed Missile System Division had been chosen as the FBM system program manager, and they were then given a contract by SPO in April 1957 'to determine the suitability of missile development for submarines'.[69] They were chosen to replace Chrysler Corporation as the missile contractor.

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66. Wilkes and Gleditsch, especially Chapter 11.
67. See The First Forty Years (Silver Spring, Maryland: Johns Hopkins University Applied Physics Laboratory, 1983), 109-17.
68. The Transit programme is discussed in more detail in Chapter 4.
69. Lockheed Missiles & Space Company, Inc., Fleet Ballistic Missiles - 25 Years (Sunnyvale, Calif.: Lockheed Missiles & Space Company, Inc., n.d.), 8.

LMSD had already developed a three-stage solid-propellant ballistic missile, the X-17, as a test vehicle for the Air Force.[70] Using this they were able to begin a series of flight tests for the FBM programme almost immediately - the first was on 11 January 1957.[71] The key technologies which were required for the Polaris missile subsystem included: a high impulse, large diameter solid propellant; a casing for the missile; a method of steering the missile; a means of terminating the thrust; and a light enough payload - warhead/re-entry vehicle combination - that would survive atmospheric re-entry.

Developments in solid propellant technology during 1955 and 1956 had been central in SPO's successful campaign for its own missile. Two kinds of solid propellant were under development. Double base cast propellants had been developed under OSRD sponsorship in World War II and combined nitrocellulose and nitroglycerine in a solvent producing a mixture which could then be extruded to the desired size. In the 1950s double base propellants became widely used in JATOs (jet assisted take-off systems) for boosting missile or aircraft take-off. They also were used in the the Talos and Terrier class missiles, which were developed under Navy sponsorship by the Allegany Ballistics Laboratory and Johns Hopkins Applied Physics Laboratory. However, the resultant propellant was very hard and inelastic and could not be case-bonded into a missile casing.[72]

The alternative, composite propellant technology was more recent. Rubbery compounds such as polyurethane were combined with ammonium perchlorate oxidizer, and allowed relatively fluid casting. In late 1955 scientists working under a Navy Bureau of Ordnance contract at the Atlantic Research Corporation made an important breakthrough. By adding large amounts of aluminium powder to the propellant they obtained a significant increase in specific impulse.[73] Aerojet Corporation was the main manufacturer of composite propellant and they built the

70. The X-17 was the launch vehicle used to explode nuclear warheads at an altitude of 300 miles in PROJECT ARGUS.

71. 'A History of Lockheed', Lockheed Horizons, Issue Twelve (Burbank, Calif.: Lockheed Corporation, 1983), 81.

72. Interview.

73. See Baar and Howard, 32-3.

rocket motors for both the first and second stages of the Polaris A1. Meanwhile, in the search for higher specific impulse, SPO also sponsored continuing research on double base type propellants with the Hercules-Allegany Ballistics Laboratory team.[74]

Aerojet cast their composite propellant into steel cases for the first Polaris missiles. Simple in concept, each missile casing nevertheless had to cope with the high temperatures and pressures created by the burning propellant. To meet these demands whilst improving weldability a low-alloy steel was specially developed for Polaris.[75] By reducing the weight of the cases and increasing the propellant thrust, SPO could now work on increasing missile range.

However, for the first Polaris range increases were not crucial once about 900 nautical miles had been obtained - that was enough to demonstrate feasibility and consolidate the FBM programme. Methods of controlling the direction of the thrust vector and of terminating thrust in the second stage were more critical, if the purported deterrence role of threatening Soviet cities was to seem credible. Without thrust vector control the trajectory of the missile could not be changed after launch. The preliminary flight tests indicated that a number of approaches might be feasible. One was simply to use jet vanes, and this remained the back-up option. A more elegant solution was the 'jetevator', invented by the former V1 scientist, Dr Willy Fiedler.

The jetevator was basically a solid ring with a spherical inside surface which was hinged over the rocket nozzle. When turned into the exhaust stream it deflected the flow. The two Polaris A1 stages both had four nozzles with jetevators to simplify the flight control formulations. Nevertheless, the corrosive nature of the exhaust stream, and its high content of aluminium oxide, led to considerable problems in getting the jetevators to work reliably during the static motor tests.[76]

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74. E. H. Kolcum, 'First Polaris Launched From Submarine', Aviation Week, Vol. 73 (July 25, 1960), 32

75. G.G. Whipple, 'Power for Polaris', Ordnance (January-February 1962), 583-85 at 584.

76. Fuhrman, 275.

The other critical issue was thrust termination. The whole purpose of the rocket stages was to bring the payload - the warhead and re-entry vehicle - to a point in space where it would fall ballistically to its target. Just as the missile reached the desired velocity its second stage rocket needed to be turned off instantaneously. This was a relatively easy task in liquid-fueled missiles, but not in solid-fueled ones. One way of doing this was simply to blow the nozzles off, so causing a rapid drop in pressure that would extinguish the motor, but with such large motors this looked difficult to make reliable for all the conceivable ranges and the large shock wave caused was felt likely to upset the missile electronics.[77]

That and various other conceptual methods of thrust termination lost out to a system in which vents were opened at the front of the second stage - the escaping exhaust gases causing it to reverse at the time of separation from the payload. Six ports were built into the front of each second stage with plugs that could be pyrotechnically removed when required. Although more laborious and expensive than simply pyrotechnically cutting ports through an homogeneous steel case, this approach was considered likely to work well without causing undesirable shock levels.[78] At this stage all other considerations were subsumed to demonstrating feasibility on schedule.

Lockheed also had responsibility for developing the payload carried by Polaris. Lawrence Livermore's predicted small warhead needed to be built along with a protective shield to prevent damage during atmospheric re-entry. With payload weight critical because of its large effect on range, it was decided that the only way to keep the payload sufficiently light would be to integrate the warhead and re-entry system. Current design practice, as used in the other ballistic missile programmes, was to build the warhead and re-entry shield as separate entities.

Headed by Lt. Robert Wertheim, the re-entry section of SP-27 coordinated this work, with Lockheed responsible for re-entry vehicle design, Livermore for warhead design, and the Naval Ordnance

77. Interview.
78. Ibid.

Laboratory, White Oak for arming and fuzing devices.[79] Teller's warhead prediction had in fact been something of a 'guesstimate' and Livermore were unable to meet the original yield goal·on schedule. However, the megaton goal was not considered critical at SPO, whereas schedule was. In July 1957 the Atomic Energy Commission detonated an experimental warhead design which confirmed the general feasibility of a Polaris warhead.[80] By the time Polaris A1 was first deployed in 1960 a yield of the order of 450 kilotons had been achieved in the W-47 warhead.[81] This comfortably exceeding SPO's internal minimum satisfactory yield of 300 kilotons.[82]

Designed to be integral with the warhead - that is, to share structural and functional components - the re-entry vehicle needed to be able to protect it from the rigours of atmospheric re-entry. The choice had to be made between the two main technical approaches to this challenge. The approach then favoured by the Army, and used in the Jupiter IRBM, was to dissipate re-entry heating by the *ablation*, or 'burning off' of the outer layers of the re-entry vehicle. The Air Force, on the other hand, initially favoured *heat sink* re-entry vehicles whose metallic construction simply absorbed (and partially re-emitted) the heat. Although they later moved to ablative re-entry vehicles for ICBMs the Air Force used the heat sink type in their Thor IRBM.[83] SPO also chose to use a heat sink design for Polaris, mainly because it looked likely to require less flight testing to validate the design. As Admiral Smith recalls:

> The Jupiter re-entry system was an ablative cooling system, as opposed to a heat sink, and the estimates that had been made by Bothwell and later updated by Lockheed were also based on that ablative cooling assumption. There's no doubt that it's . . . proven a very efficient, weight efficient method of disposing of the heat generated in re-entry. However, as I saw it, at least, it has the disadvantage that the amount of ablative material cannot be reasonably validated without flight tests. . . . So when Lockheed . . . proposed a quite different shape, and a heat sink approach,

79. Fuhrman, 270.
80. Ibid, 275.
81. Various sources give the yield of the first version of the Polaris warhead, the W47-Y1, as between 450 to 600 kilotons.
82. Interview.
83. See M. A. Armacost, The Politics of Weapons Innovation (New York: Columbia University Press, 1969), 144-46.

> which could much more reasonably be calculated . . . I favoured that approach.[84]

A problem then with heatsink designs was 'the extreme aerodynamic heating associated with high-speed re-entry of streamlined, slender cones.'[85] To avoid this most re-entry vehicles at the time were blunt designs whose slower descent reduced heating, but also made them more susceptible to wind drift inaccuracy.[86] For Polaris beryllium was chosen for the heatsink because of its 'light weight, high strength-weight ratio at high temperature and high specific heat'.[87]

Consequently, SPO achieved their goal of a warhead/re-entry vehicle combination weighing less than 900 pounds.[88] Whilst other services were combining warheads weighing some 1500 lbs with re-entry vehicles to give total weights over 3000 lbs, Teller's promised warhead was of the order of 600 lbs, which when designed into an integral re-entry vehicle produced a total of weight of about 850 lbs.[89] This was designed to be ejected from the second stage motor at the time of thrust termination by an airspring. A flare section attached to the rear of the re-entry vehicle provided 'aerodynamic damping at re-entry and stabilization during descent', and contained gas nozzles which spun the re-entry vehicle to provide stability and symmetry during re-entry.[90]

However, although the Polaris payload was an innovative success it was not without problems. Just prior to the nuclear test moratorium began in October 1958, Livermore conducted a 'one-point' safety test on the W-47, which surprisingly gave a yield of about 100 tons.[91] Because the

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84. Interview.

85 Aviation Week, Vol. 68 (March 17, 1958), 24.

86. Heating is reduced because the blunt re-entry vehicle creates a detached shock wave in advance of it which dissipates most of the energy of re-entry.

87 Aviation Week, Vol. 68 (January 6, 1958), 39.

88 Fuhrman, 270.

89. Interview.

90 Fuhrman, 272.

91. Statement of Roy D. Woodruff, Before the Subcommittee on Arms Control and Disarmament, Armed Services Committee, US House of Representatives (September 20, 1985), mimeo, 11. See also Chuck Hansen, US Nuclear Weapons: The Secret History (Arlington, Texas: Aerofax, 1988), 204; T. B. Cochran, W. M. Arkin, R. S. Norris and M.M. Hoenig, Nuclear Weapons Databook Volume II: US Nuclear Warhead Production (Cambridge, Mass.: Ballinger, 1987), 48, who define one-point safety as requiring 'that the probability of achieving a nuclear yield greater than four pounds of TNT equivalent shall

moratorium prevented the tests considered necessary to develop a design that was inherently one-point safe, a mechanical safing system was incorporated. This, however, failed to operate satisfactorily, even after several fixes and in many instances the mechanism would have failed to arm the warhead resulting in a dud.[92]

Submarine Construction

Getting submarines ready to carry Polaris was the responsibility of the ship installation branch of SPO, SP-26. When the missile development was accelerated in December 1957 it was decided that the long development time of an all-new FBM submarine would delay deployment. On December 30, Electric Boat were awarded a contract for the design and construction of the first submarine, the USS *George Washington*, which was in fact a conversion of an attack submarine, the USS *Scorpion*, already under construction.

With submarine construction already the jurisdiction of the Bureau of Ships, SP-26 thus had a rather different and potentially more difficult role than the other technical branches, who could deal directly with contractors under freshly instigated arrangements. Whereas guidance, navigation, launcher and missile developments were assigned to 'prime contractors' all keen for new business and willing to match the new technology with new management approaches,[93] SP-26 had to deal with BuShips bureaucracy and the traditional management techniques of their construction companies. To make matters potentially even more fraught, nuclear-powered vessels required BuShips themselves to work together with Admiral Hyman Rickover, whose power derived not only from his technical expertise and position as head of the Navy's Nuclear Propulsion Directorate, but also from his other role as director of the AEC's Naval Reactors Branch and his strong support in Congress.

not exceed one in one million in the event of a detonation initiated at the single most sensitive point in the high explosive system'.

92. Interview.

93. Except perhaps for Sperry, who became navigation integrator in 1958 and were a traditional Navy contractor.

However, at this crucial stage in the submarine development Rickover's ability to interfere was minimized by Burke's firm control, and in any case SPO were able to convince him that his interests (getting nuclear power into the US Navy) were closely tied to the success of the Polaris submarines.[94] Indeed Rickover's technical prowess and drive made him a formidable ally in the construction of the first Polaris submarines, once the danger of him attempting to alter the design had been alleviated. Although the construction of the first FBM submarine, the *George Washington*, was 'plagued by scores of mistakes' and 'many goofs' it was still completed at the Electric Boat yard on schedule and launched on June 9, 1959.[95]

Testing Polaris

During 1957 and 1958 flight tests continued using an assortment of available rocket boosters to assess the chosen methods of thrust termination and vector control, and to investigate re-entry vehicle thermodynamics. A total of twenty-two of these FTV (flight test vehicle) flights were carried out.[96] In September 1958 the first of seventeen Polaris prototype AX series test flights began, with initially not very impressive results.[97]

The first took off satisfactorily, but continued to climb vertically, apparently because of a malfunction in the autopilot programmer used in place of the inertial guidance system.[98] In the second test flight the first stage rocket malfunctioned and the missile never left the pad. AX-3 and AX-4 demonstrated erratic behaviour during the first-stage flight, caused by overheating at the base of the stage and consequent malfunction of electrical wiring. This led to an intensive investigation to understand the problem, 'Operation Hotfoot', and to corrective changes in 'Operation Phoenix' which had remedied it by AX-6.[99] Of the seventeen AX flights,

94. See Chapter 4.
95. Baar and Howard, 139.
96. Fuhrman, 275.
97. For a complete listing of AX tests, see 'Longer Range Promised Through Improved Motors', Missiles and Rockets, Vol. 7 (July 25, 1960), 20-23, at 23.
98. Fuhrman, 276.
99. Ibid.

five were classed as successful, eleven partially successful, and one (AX-2) as a failure.[100] 'Partial success' was, however, something of an SPO euphemism since any test that returned some useful data was not considered a complete failure.[101]

In September 1959 the A1X flight tests began using hardware very similar to that of the Polaris A1 missile later deployed. The MIT inertial guidance system was first introduced in January 1960 and from flight A1X-14 onwards the hardware flown was 'substantially the same as the production design except for the added instrumentation and range safety provisions'.[102] Forty A1X flight tests were carried out with twenty-eight evaluated as complete successes, eleven as partially successful, and only one a complete failure.[103]

A1X-31, however, was a particularly important test, the first from a submerged submarine. On 20 July 1960 it was launched successfully to a range of about a thousand miles from the USS *George Washington.* A few hours later a second Polaris missile was launched, again with complete success. After many set-backs SPO could breathe a sigh of relief. Admiral Burke's faith in the Polaris project had been justified. The Polaris concept was now technology, and it worked!

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100. Ibid, 277.
101. See 'Polaris A3 Reaches Advanced Test Phase', Aviation Week & Space Technology (May 4, 1964), 16.
102. Fuhrman, 277.
103. Ibid.

Chapter 4

Success and Successors

This is not an ultimate missile here. We are going to keep improving this missile as we go along, even after it is first installed in the ships, so we are not going to get an ultimate missile and stop.

Admiral Burke.[1]

Polaris A1 became operational on November 15, 1960, when the submarine *George Washington* left Charleston, South Carolina to patrol the Norwegian Sea. Then in December the second FBM submarine, the *Patrick Henry,* went on patrol. Each carried sixteen Polaris A1 missiles capable of delivering a nuclear warhead over a range of about a thousand miles to within a few miles of the intended target. Polaris seemed to be an indisputable success.

The Social Construction of Success

Within four years SPO had developed and deployed a complex, new type of weapon system which provided a threat of potential retaliation against Soviet cities, but which itself seemed invulnerable. This success owed much to the skill and dedication of the people that worked on the programme. In particular SPO demonstrated great skill in managing *both* the 'technical' and 'social' aspects of technology. Moreover, within certain limits they were able to 'engineer' the expectations that Polaris had to meet just as well as the technology that met them.

Schedule was paramount, with a sense of urgency generated not only by concern about the need to counter possible Soviet developments, but also to establish a Navy right to ballistic missiles before the Air Force achieved the hegemony it clearly desired. To meet the schedule other system parameters could be traded off. Thus the A1 initially fell somewhat

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1. Quoted in Robert E. Hunter, 'Politics and Polaris: The Special Projects Office of the Navy as a Political Phenomenon' (unpublished Senior Honours Thesis, Wesleyan University, June 1962), 272.

short of the 1200 mile range goal and carried a warhead with a yield about half the one megaton goal. Neither trade-off mattered since SPO was able to argue that Polaris A1 still provided a useful interim deterrent capability. Likewise SPO set a pattern for future systems by agreeing to an accuracy figure as a *goal* and not as a *requirement.*

For the original Polaris this goal was 'a couple of miles CEP'*, 'about the size of a city' which at the start of the programme seemed to many to be 'probably ... unobtainable' because of the then state-of-the-art of ship navigation.[2] Even a four mile CEP would have been considered satisfactory, but participants recollect that accuracy goals were met 'as far as we could tell'.[3] Polaris A1 probably averaged an accuracy better than two miles. But at the time, before the geodetic mapping carried out using Transit, knowledge of the precise location of targets was itself a major inaccuracy. For example, in 1959, before Transit, 'the location of Australia was wrong by several thousand meters'.[4]

What mattered was demonstrating feasibility: that it could be built, that the components would function individually and collectively. Within broad limits its precise characteristics, such as accuracy, mattered less than this overall question of whether it would, in a general sense, 'work'. If the CEP of Polaris A1 had turned out to be ten miles, or twenty miles, then it might have been judged a failure, but anything under five was quite adequate. The Naval Warfare Analysis Group's first study of the FBM, distributed in January 1957, emphasized the flexibility of performance characteristics: 'Requirements for yield and accuracy should be subordinated to early availability of the weapon'.[5]

This relaxed attitude to accuracy, as to other performance criteria that were not considered critical, helped ensure the success of the programme. SPO realized the importance of demonstrating feasibility on schedule, and avoided any unnecessary 'requirements'. To meet

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2. Interviews.
3. Interview.
4. Richard B. Kershner, 'Technical Innovations in the APL Space Department', Johns Hopkins APL Technical Digest, Vol. 2 (Jan-March 1981), 264-78, at 269.
5. Naval Warfare Analysis Group Study No. 1, 'Introduction of the Fleet Ballistic into Service '(January 1957), Serial 007P93, 7.

* CEP - circular error probable - is defined as the radius of the circle around the target within which 50% of warheads would be expected to fall.

schedule, as Harvey Sapolsky has noted: 'Performance was a manipulatable variable in the Polaris program'.[6] Moreover, whilst SPO was developing the weapon that would provide an 'assured destruction' retaliatory threat to cities, Admiral Burke and others were developing the strategic logic that would require it. High accuracy looked to be beyond foreseeable technology in a sea-based ballistic missile (and to be inherently easier for the land-based Air Force ICBMs) and so it was not 'needed'.

However, the success of Polaris cannot be attributed entirely to the heterogeneous engineering of its proponents, highly skilled though this was. Polaris also benefited from the wider social context of the day, a context which partly lay beyond the influence of SPO and their collaborators. Polaris was a success because its need was perceived to be so great and so urgent at the time. But the national paranoia following Sputnik was something which would probably have occurred if SPO and the Polaris programme had not existed, as indeed would Senator Kennedy and the 'missile gap' mythology he exploited in his bid for the Presidency.

Nor did the success of Polaris go completely unchallenged. During its development there had been criticism from Air Force sources suggesting that the FBM concept was beyond the state-of-the-art in technology, especially in the area of submarine navigation.[7] By attacking the feasibility of Polaris the Air Force sought to counter the challenge that it constituted to their control over strategic weaponry:

> The Air Force could see a fight ahead for dollars that formerly had "Air Force" written all over them. Therefore, the Air Force missed few opportunities to remind the administration that Polaris was unproved.[8]

But as Polaris tests progressed the credibility of the mainstream arguments against its feasibility began to weaken. Although the validity of the tests, and what they actually demonstrated, remained open to question, in practice the Air Force could not do so without raising the same doubts

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6. Harvey M. Sapolsky, The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972), 141.
7. Interview.
8. James Baar and William E. Howard, Polaris! (New York: Harcourt, Brace & World, 1960), 198.

about its own comparable ICBM and IRBM testing. Even in the area of navigation - where the technical challenge *was* significantly different for a sea-based system - Navy demonstrations provided convincing evidence that adequate accuracy could be achieved. And, of course, Burke and SPO were promulgating a strategic doctrine in which 'adequate accuracy' to threaten urban-industrial targets was not only sufficient, but even preferable to 'pinpoint' accuracy.

By carefully differentiating their targeting doctrine from that of the Air Force, Polaris supporters thus made irrelevant such doubts raised by the Air Force about the ability of Polaris to destroy hardened military targets:

> It is still unclear, however, how efficient the low-payload Polaris will be against hardened targets, for no prototype missile has yet been fired, nor has the radically new ship's inertial navigation system - crucial to accurate firing - been perfected.[9]

By instead stressing the counter-city role of Polaris - retaliation against large, 'soft' targets - it was possible to deflect such criticism. It also left the Air Force able to justify its ICBM and bomber forces by reference to their primary counterforce mission (hence the concentration on improving ICBM hard target effectiveness from the early 1960s on), and so reduced their need to criticize Polaris. This did not stop the interservice fight over ballistic missile control and funding, but it differentiated the Air Force and Navy programmes sufficiently to allow for neither side to lose out completely as they could be seen as complementary rather than direct competitors.

Indeed the attempt during 1959 and 1960 by the Air Force to obtain jurisdiction over Polaris was a tacit acceptance of its technical feasibility and strategic legitimacy. Technical criticism specific to a sea-based ballistic missile was largely laid to rest by a RAND Corporation analysis of Polaris produced in October 1958 in response to an Air Staff request for a 'factual

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[9]. C. E. Selberman and S. S. Parker, 'The Economic Impact of Defense', Fortune, Vol. 57 (June 1958), 215. Cited in Hunter, 145. See also Baar and Howard, 215-6; D. A. Rosenberg, 'Arleigh Albert Burke' in R. W. Love, Jr., The Chiefs of Naval Operations (Annapolis, Maryland: Naval Institute Press, 1980), 310-12.

and unbiased assessment'.[10] (But see below for continued opposition to *all* ballistic missiles by Air Force bomber stalwarts.) Thereafter, the Air Force's attitude to Polaris centred on the targeting issue. Burke's minimum deterrence, counter-city strategy was actively criticized as the Air Force sought to justify its own preference for counterforce.[11] But at the same time Strategic Air Command leaders sought to gain control of Polaris by proposing the integration of all US strategic forces for the purpose of coordinating targeting.

This, of course, met fierce resistance, as a 'high ranking' naval officer remarked:

> Polaris is perhaps the most attractive missile system under development. ... Of course they want control of Polaris. But they will have to walk over a prostrate Arleigh Burke to get it.[12]

The takeover attempt did not succeed, but the Air Force move did result in the coordination of targeting into a Single Integrated Operational Plan (SIOP) in 1960. Since the Joint Strategic Target Planning Staff set up to produce the SIOP was dominated by Strategic Air Command officers it effectively settled the interservice argument over targeting.[13] The SIOP embodied the Air Force belief in a massive (and preferably pre-emptive) attack against the complete range of counterforce and urban-industrial targets. Air Force criticism of Polaris was now muted though the much vaunted invulnerability of Polaris was still publicly questioned by the Air Force.

But in contrast to the previous decade the early 1960s saw the Air Force very much on the defensive, attempting to protect their budget share from the rationalization plans of President Kennedy's new Secretary of Defense, Robert McNamara. The main focus of Air Force/Navy competition shifted to the issue of cost-effectiveness which now came to

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10. Quoted in D. A. Rosenberg, 'The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960', International Security, (1983), 57.
11. Ibid, 58-60.
12. Quoted in Ibid, 61.
13. The JSTPS comprised 219 SAC personnel, 29 Navy, 10 Army, 3 Marine and 8 additional non-SAC Air Force. Ibid, 5.

dominate Pentagon thinking.[14] Now that the reputations of SPO and Polaris were established, Navy support for the proposed B-70 bomber was no longer forthcoming in 1961 and it was canceled despite attempts to sell it as an alternative mobile ballistic missile basing system for the Skybolt air-launched ballistic missile.[15] Likewise the Air Force pushed the mobile Minuteman in an attempt to compete on the invulnerability issue:

> The railroad-based Minuteman ... could elude the enemy in the event of hostilities and avoid a first strike in the same way that the Navy's Polaris missile submarines can evade detection.[16]

The Air Force argued that their new Minuteman ICBMs would be much cheaper to deploy than Polaris. According to McNamara's own figures in 1961 the cost for each Polaris missile on station would be $9.7 million, whereas a mobile Minuteman would cost $5.0 million, and a fixed-base Minuteman only $3.2 million.[17] SPO countered this by stressing that the invulnerability of Polaris made it more cost-effective than Minuteman and that Polaris was already available, whereas Minuteman had yet to be tested. Doubts about whether the Air Force would deliver Minuteman on cost and schedule were also echoed by Budget Director Stans:

> ... we were not quite certain that the development of the Minuteman could be successfully accomplished with the level of costs estimated by the Air Force. Or whether or not the timetable could be wholly relied upon. And finally, we had no estimate of the cost of hardening the Minuteman system that we could rely upon.[18]

Polaris was also looked on very favourably within McNamara's OSD, where it came out well in the cost-effectiveness calculations.[19] Herbert York, the first Director of Defense Research and Engineering, who

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14. A good account of McNamara's new management philosophy and methods can be found in Alain C. Enthoven and K. Wayne Smith, How Much Is Enough? Shaping the Defense Program, 1961-1969 (New York: Harper & Row, 1971).
15. See testimony of Admiral Burke quoted in Hunter, 149.
16. Quoted in Hunter, 155.
17. Cited in Hunter, 146.
18. Quoted in Hunter, 147.
19. Fred Kaplan, The Wizards of Armageddon (New York: Simon and Schuster, 1983), 254-5.

stayed on briefly from the Eisenhower administration, considered 'Polaris to be one of the really well run programs in the Defense Department'.[20] Although the Kennedy administration soon discovered that pre-election rhetoric about the 'missile gap' was erroneous, it remained politically expedient to bolster strategic forces.[21] Polaris became a major beneficiary of this. The Air Force alternatives were either considered somewhat obsolete already (the liquid-fueled Atlas and Titan ICBMs) or unproven (the solid-fueled Minuteman). Although a Minuteman force of 1000 was approved by McNamara, this fell far short of Air Force demands, and the mobile Minuteman was first deferred and then canceled in December 1961.

However, following the successful test flights of Polaris and of other US ballistic missiles, a different, more general challenge to their feasibility emerged. A group of critics, centred around Air Force bomber officers, began to argue that although the flight tests might demonstrate the feasibility of certain components under test conditions, they did not demonstrate the effectiveness of the system under 'real' operational conditions.

In what was apparently an attempt to settle this question the Navy carried out a 'live' test of a Polaris A1 on May 6, 1962 - the only such test performed by a US ballistic missile. Known as 'Operation Frigate Bird' this involved launching a missile from the USS *Ethan Allen* over 1000 miles to the nuclear testing ground at Christmas Island. The test was considered a resounding success, and was reported to have 'hit "right in the pickle barrel"' exploding with a yield estimated at half a megaton.[22] Ironically this was the W-47 warhead incorporating the faulty mechanical safing device which was later estimated to have perhaps a fifty per cent chance of producing a dud.

In any case the 'Frigate Bird' test did not entirely mollify missile critics and the Partial Test Ban Treaty signed in 1963 prevented a repeat.

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20. Interview.

21. See Desmond Ball, Politics and Force Levels: The Strategic Missile Program of the Kennedy Administration (Berkeley: University of California Press, 1980).

22. 'Live Polaris Launch', Aviation Week (14 May, 1962), 35; also Robert A. Fuhrman, 'The Fleet Ballistic Missile System; Polaris to Trident', Journal of Spacecraft, Vol. 5, No. 5 (Sept-Oct 1978), 277.

Public criticism of ballistic missile reliability continued through 1964 with both Senator Barry Goldwater (an Air Force reserve Major General who had 'long identified himself with the bomber faction'[23]) and Air Force Chief of Staff (and former head of SAC) General Curtis LeMay voicing their doubts. However, such criticisms were difficult to sustain when many of the main critics were also institutionally committed to ICBMs. As Air Force Chief of Staff, LeMay was at the same time arguing for a force of several thousand Minuteman ICBMs. In the end the argument over whether ballistic missiles would actually work lost credibility not because tests proved that they would, but because these influential critics ceased to argue that they would not.

The success of Polaris was thus not simply a technical matter. Whether the missiles would actually work in a nuclear war was, *and is*, an issue that can always be questioned in principle. In practice, however, SPO developed an unrivalled reputation for producing what they had committed themselves to. If any US ballistic missile was going to work then SPO's impressive managerial style and avoidance of technical over-elaboration suggested that it would be Polaris. But Polaris A1 was only SPO's first step in the development and establishment of the FBM system.

Polaris A2

The Polaris A2 was developed almost simultaneously with the A1, with the understanding that the A1 schedule was paramount. Meeting this schedule resulted in a production missile which fell short of the range and warhead yield initially hoped for, and which had low reliability.[24] It had 'worked' very effectively, in that it had demonstrated the feasibility of the concept and staked out the Navy's claim to it. But its reliability was not considered very good, as one Lockheed manager recalled:

> [the] Polaris A1 propulsion system was inherently not reliable. ... Polaris A1 was a vehicle that had been

[23]. Anon., 'Fixes planned for Minuteman deficiencies', Aviation Week and Space Technology (February 3, 1964), 26-7.

[24]. R. Lindsey, 'B-3 Polaris Expected To Be Operational in '70', Missiles and Rockets, Vol. 15 (August 24, 1964), 28, quotes Lockheed Missiles and Space Corporation general manager Stanley Burris as saying that Polaris A3 was 'at least 25 to 30%' more reliable than A2, which was a similar advance over A1.

designed to be flight tested by engineers. It was then turned over to the Navy as a weapon. It demanded, but you couldn't give it, tender loving care in all electrical connections. And so it had electrical failures, no one failure mode predominating, that is no one area predominating, just lots of them. ... A1 was a hell of a fine weapon considering the circumstances in which it was done but it was not acceptable when it was possible to have the A2.[25]

The main changes in A2 were a new second stage to provide longer range, an improvement of the warhead design to provide slightly greater yield, and more reliable electronics. Originally the increased range was to have been achieved by increasing propellant energy and reducing inert weight throughout the missile. However, the much greater effect of improvements in the second stage, coupled with a conservative attitude to changing too much of the design at once, led to a decision to retain basically the same technology in the first stage, with an alternative second stage design. The first stage was simply increased some thirty inches over the A1 first stage to take advantage of space originally set aside for the launcher's buoyancy compensation tanks, but used the same propellant formulation and case construction as in A1.

The alternative higher performance second stage was the result of work that SPO had sponsored at the Hercules-run, government-owned Allegany Ballistics Laboratory. This had come about because Admiral Raborn had wanted a backup for the original Polaris. Technical Director Levering Smith had then argued that the backup 'should be different in every respect. It should have a completely different approach to the propellant, have a completely different approach to the case, and the thrust vector control system should be different.'[26] A double base (nitrocellulose/nitroglycerine) propellant using ammonium perchlorate oxidizer and aluminium fuel was developed that could be cast into rocket cases. Full scale development was given the go-ahead in June 1958.[27]

Hercules Powder Co. were also responsible for pioneering the development of fibre-glass as a rocket case material. This was apparently originally stimulated by fear that a pending strike at the steel producer

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25. Interview.

26. Interview.

27. Fuhrman, 278.

would jeopardize the programme.[28] Fibre-glass also appeared to offer higher strength-to-weight ratios than steel. Development of the second stage chamber for the Polaris A2 was started in 1958, with the first flight in November 1960.[29]

The alternative approach also considered thrust vector control. This sought to overcome the problems that the jetevator had experienced, to reduce the inert weight of the vector control system and minimize the loss of axial thrust in steering. Out of the several concepts investigated a rotatable nozzle design, similar to that already proving successful in the Air Force Minuteman ICBM programme, was chosen for use in the second stage.[30]

Polaris A2 was initially deployed with the half megaton W47 (known as W47-Y1), but this was later replaced by a W47-Y2 with a yield close to the original one megaton goal.[31] This upgrade was obtained during the 1958-62 nuclear test moratorium by replacing the secondary device (the fusion part of a hydrogen bomb) of the existing design, but this version of the W47 still incorporated the faulty mechanical safing device. The problem was eventually remedied by the replacement of the warhead primary in 1967.[32]

Thus the A2 provided very similar, but slightly enhanced performance as compared to A1. But whereas 'the thing that drove A1 was to get there as soon as possible, the thing that drove A2 was improve the reliability'.[33] Both were considered to have the same strategic mission, to target Soviet cities, and hold them to ransom against communist aggression by the threat of retaliatory devastation. A2 was what the Navy had originally promised to provide for this purpose, but A1 allowed them

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28. Lockheed Missiles & Space Company, Inc., Fleet Ballistic Missiles - 25 Years (Sunnyvale, Calif.: Lockheed Missiles & Space Company, Inc., n.d.), 4.
29. Ibid.
30. See 'Longer Range Promised Through Improved Motors', Missiles and Rockets, vol. 7 (July 25, 1960) 20.
31. Interview.
32. Statement of Roy D. Woodruff, Before the Subcommittee on Arms Control and Disarmament, Armed Services Committee, US House of Representatives (September 20, 1985), mimeo, 11. The W-47 warhead also suffered from problems with corrosion of its fissile material. See Chuck Hansen, US Nuclear Weapons: The Secret History (Arlington, Texas: Aerofax, 1988), 204-05.
33. Interview.

to provide a lesser, but conceptually similar, capability sooner. It also helped consolidate the Navy's role in ballistic missile weaponry. The first successful submerged launch of the A2 was from the USS *Ethan Allen* on October 23, 1961, and the A2 became operational in June 1962.[34]

Polaris A3

With the A1 and A2 programmes on schedule, SPO began to consider another generation of Polaris in 1959. Initially the obvious approach seemed simply to increase the range yet further and to use a larger warhead, as was desired by a faction in the Office of the Chief of Naval Operations, who favoured attempting to match the counterforce role of the Air Force ICBMs.[35]

However, a number of factors militated against this option. Firstly, the nuclear test moratorium still prevented testing of new warhead designs, and a larger warhead was only available as an adaption of one designed for Air Force use, or by scaling up a smaller yield design. Secondly, the Soviet Union (like the USA) had started the development of anti-ballistic missile systems which were intended to provide some defence against ballistic missile attack by intercepting them in flight. As concern grew some provisions were made to carry penetration aids on the already deployed Polaris missiles, and electronic countermeasures developed by Lockheed Electronics and Raytheon were flight tested.[36]

SPO's design for the Polaris A3 neatly combined these factors to counter expected developments in Soviet ABM defences, whilst meeting the original one megaton goal of Polaris without using warhead designs that were either untested or of Air Force provenance. Instead of the single warhead/re-entry vehicle combination carried by Polaris A1/A2 and all other ballistic missiles of the time, SPO again broke new ground in developing a multiple re-entry vehicle (MRV) system.

34. Strategic Systems Program Office, FBM Facts/Chronology - Polaris, Poseidon, Trident (Washington, DC: Navy Department, 1986), 28.
35. Interview.
36. B. Miller, 'Studies of Penetration Aids Broadening', Aviation Week & Space Technology (January 20, 1964), 87.

The warhead used was a 200 kiloton design known as Tuba which the Livermore laboratory had developed and tested prior to the moratorium.[37] Livermore offered to scale this up to the one megaton goal, but because it could not be tested at that yield SPO declined, preferring to use the tested version. Instead the A3 missile was designed to carry three of the 200 kiloton warheads, which weighed about 200 lbs, providing an equivalent megatonnage roughly the same as a single one megaton warhead.[38]

A new re-entry vehicle, the Mk2, was developed to carry the W58, as the Polaris version of Tuba was designated. This differed significantly from the Mk1, using a 'nylon-phenolic ablative heat shield' over an aluminium structure instead of a heatsink design.[39] The reason for this was that at the longer 2500 miles range of the A3, heatsink re-entry vehicles needed to be increasingly larger to absorb the extra heat of re-entry and consequently the lower weight (not to mention lower expense) of ablative designs became more attractive.[40] Moreover, with the Polaris A1 and A2 deployed, schedule was no longer quite so urgent and by now the ablative approach had been validated, not only by continuing SPO sponsored research, but also by Air Force and Army developments.[41]

The Mk2 re-entry vehicles were to be released simultaneously from the missile when the correct velocity was reached to take them to their target area. This separation was achieved by small solid-fuel rockets which separated the re-entry vehicles from the missile second stage, and spun them (for stability).[42] The second stage simply carried on till burn out and

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37. Interview; also T. Greenwood, Making the MIRV: A Study of Defense Decision Making (Cambridge, Mass.: Ballinger, 1975), 161.

38. Equivalent megatonage is a measure of the destructive effect against a large, 'soft' target, such as a city. If y is the yield of a warhead, its equivalent megatonnage is y 2/3. The blast, etc., from a nuclear explosion is propagated outwards in what is roughly a sphere, so the 'lethal radius' for any immediate effect is approximately proportional to the cube root of the yield. Area damage is therefore roughly proportional to the square of the cube root - i.e. y2/3.

39. Fuhrman, 280.

40. Interview.

41. See M. Yaffee, 'Ablation Wins Missile Performance Gain', Aviation Week (July 18, 1960), 54-65; and 'Pyrolytic Graphite Studied for Re-Entry', Aviation Week (July 25, 1960), 26-28.

42. Fuhrman, 280; Interview.

the need for a thrust termination system - with its likely addition to deployment inaccuracy - was thus avoided in A3.

The angle of separation of the re-entry vehicles from the missile was determined by the perceived nature of Soviet ABM developments. This perception was very general, however, and so design criteria were based on the current American state-of-the-art in ABM interceptors, the Army's Nike-Zeus missile. The separation had to be small enough for all three warheads to explode over the same target area, but large enough to prevent a single Nike-Zeus type destroying or disabling all three with the radiation blast from its nuclear warhead. In practice the separation achieved led to a 'footprint' at the target area about a mile across, depending, of course, on the exact distance traveled.[43] Because of this warhead separation the A3 was known as 'The Claw'.

Concern about Soviet ABM defences also inspired Navy work on penetration aids for the A3. Work was done to develop decoys that could mimic the appearance of real re-entry vehicles (as far as the ABM radars were concerned), but be a fraction of the weight. Such light decoys could only 'look' like real re-entry vehicles outside the atmosphere before the friction of re-entry slowed them down more rapidly than the real thing or burnt them up. However, although the Nike-Zeus was an endo-atmospheric interceptor its slow speed required it to be launched before its target entered the atmosphere. This meant that light decoys could prove very effective in confusing and overwhelming a defence based solely on Nike-Zeus type interceptors. Thus it was considered that Polaris A3 would have the option of replacing one of the Mk2 re-entry vehicles with a decoy package.[44]

However, it became clear when the Soviet Galosh ABM system was displayed at a Red Square parade in Moscow in 1964, and when its accompanying radars became operational, that the Soviet Union had not after all chosen to design an ABM similar to Nike-Zeus. Galosh was much larger, and clearly intended to carry a large warhead (exceeding a megaton) for interception outside the atmosphere. The Polaris penetration aids,

43. Greenwood, 161.
44. Ibid.

which included chaff and decoys, were designed to cope with the wrong threat:

> ... they were all cut to the wrong frequencies, they were all too small to have been seen by these low frequency radars and they were spaced improperly to accommodate the large yield weapons effects ranges of the big warheads. So other than that everything was just fine![45]

In addition to the new payload, the other main concern of Polaris A3 was increased range, with a goal of 2500 nautical miles set. This was not a figure arbitrarily imposed on SPO, but rather derived from their studies of what seemed achievable. Such an increase over the 1500-mile A2 looked possible because of the potential to reduce inert weight in the missile design whilst increasing both the amount and the specific impulse of the propellant. Moreover, there was confidence that the over-designed shock protection of the launcher system would allow a heavier missile to be accommodated.

Many of the weight-saving components planned for A3 were tested on A1 or A2 test missiles. A new approach to thrust vector control which cut down on inert weight used the injection of Freon 114 into the nozzles to deflect the exhaust stream. This was first successfully demonstrated during the second stage flight of A1X-50 on September 29, 1961.[46] It was incorporated in the design of the A3 second stage, whereas the first stage now used rotatable nozzles. Both stages were built of fibreglass and utilized denser, higher specific impulse propellant formulations.

The higher temperatures involved (over 6000°F), along with the high pressure (800-900 psi), led to difficulties with the first stage. The composite propellant developed by Aerojet for this stage was apparently more corrosive than the Hercules/Allegany Ballistic Laboratories double-base propellant used in the second stage.[47] In early tests the first stage nozzles were destroyed and it was considered necessary to reduce the

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45. Interview.

46. Fuhrman, 280.

47. 'Problems May Cut Polaris A3 Range Goal', Aviation Week and Space Technology (September 4, 1961), 31.

propellant burning temperature, thus sacrificing a little range. The final A3 first stage design used silver-infiltrated tungsten nozzle throats.[48]

A new guidance system, the Mk2, was developed, and flight tested on A2 missiles starting in November 1961.[49] To meet SPO's range goal the missile contractor, Lockheed, wanted a much smaller and lighter system, and also wanted the inertial measurement unit (IMU) to be separate from the electronics assembly (EA) so as to make it easier to pack into the limited space in the missile. Altogether the Mk2 was about half the size and one third the weight of the Mk1. Redesign of its physical structure contributed to this with the large bar beryllium gimbals replaced by smaller spherical aluminium gimbals. The pressure from Lockheed to save weight also led to the choice of magnesium for the stable member on which the inertial components were mounted. Distortion of this magnesium due to internal corrosion was to be a source of future concern for those Mk2 guidance systems kept in service by the UK Royal Navy.[50] Much smaller resolvers were also used in the Mk2 and a higher density of electronics packaging was achieved by the first use of 'cordwood' welded (rather than soldered) construction in a missile.[51]

In addition the Mk2 used smaller accelerometers (16-size - 1.6 inch diameter - rather than 25-size). Moreover, the PIGAs, Pendulous Integrating Gyro Accelerometers, were replaced with PIPAs, Pulsed Integrating Pendulous Accelerometers, on all but the most sensitive thrust axis. Unlike the PIGA with its gyro wheel and internal gimbal, the PIPA consists simply of a pendulum which is held in null position by pulses sent from a signal generator to an electromagnetic torquer. A PIPA is therefore smaller than a PIGA, and is also generally acknowledged to be a simpler, easier to produce and therefore cheaper device - although one that does not match the accuracy of the best PIGAs.[52]

Nevertheless, accuracy improvements were sought in the Mk2 guidance system, and indeed were necessary if the missile was to be as

48. Fuhrman, 280.
49. Ibid.
50. Interview.
51. B. O. Olson, 'History of FBM Guidance at CSDL' (10 March 1975, typescript), 3.
52. Ibid; Interviews.

accurate as its predecessors at the longer range. SPO, however, went further still, setting an objective of making the Polaris A3 about four times more accurate than the A2 at the longer range.[53] But improvements in technology were incorporated only in so much as they fitted the strategic and organizational goals of the programme. Increasing the missile's range, and giving it the capability to penetrate anticipated ABM defences, seemed a natural development of a counter-city ultimate deterrent. The increase in accuracy was more a consequence of lessons learnt in developing the original Polaris than a sign of a major shift to a new strategic role or any abandonment of 'differentiation' of the Navy's technology from that of the Air Force.

There had been voices calling for just that, but they do not appear to have swayed the leadership of SPO. Thus Charles Stark Draper had argued as early as 1959, in a paper to the Polaris Steering Task Group, that 'fleet ballistic missiles offer many well-known advantages, but will surely be handicapped in competition for national support unless they can be fired with accuracy levels comparable to those of land-based missiles'.[54] But, despite the desire of some in the Navy to make Polaris A3 'a warfighting machine',[55] this line of argument did not win the day. SPO did not attempt to compete with the Air Force in the accuracy stakes, and the improvements that did occur remained in the context of an assured destruction weapon. Polaris A3 was more effective than its predecessors against military targets, but only large, unhardened ones.

Two new FBM guidance technologies were developed, but not used. One, a much smaller system, called MIGIT, could have been used in a smaller missile (either for torpedo tube use or multiple packaging in the launch tube) or for re-entry body guidance. The other was a strapdown guidance system 'to duplicate the Mk2 mission requirements'.[56] As the name implies, in a strapdown system the gyroscopes and accelerometers

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53. L. Smith, R. H. Wertheim and R. A. Duffy, 'Innovative Engineering in the Trident Missile Development', Bridge (National Academy of Engineering), Vol. 10, No. 2 (Summer 1980), 10-19, at 11.

54. C. S. Draper, 'Submarine Inertial Navigation - A Review and some Predictions', paper presented to the Polaris Steering Task Group on 22 October 1959 (Draper Laboratory Library, CSD-107), 3.

55. Interview.

56. Olson, 4.

are held in fixed relation to the body of the missile. Mechanically this is much simpler, therefore presumably cheaper and more reliable, than the conventional stable platform design. Maintenance would be greatly simplified, as defective components could be 'unbolted' and replaced, no easy task in the complex, tightly-packed gimbal structure of a stable platform.

Strapdown does, however, place greater demands on some of the guidance system components. The gyroscopes experience, and have to measure reliably, much greater rotation rates. Also much more computer power is required. In a stable platform system, the inertial components are kept in a known frame of reference physically. In strapdown, the analogous task is performed computationally. 'The lack of suitably fast computers to exploit fully strapdown advantages was the major technological barrier'.[57]

As computer capabilities improved during the 1960s, strapdown became a real option, and 'a substitution for Mk2 could have been made if desired'.[58] It was not. Although not (as an organization) necessarily committed to greater accuracy, SPO however did not want a *less* accurate system, which might have been the case with strapdown, and there were also worries about the calibration difficulties involved.

In the end the only modification developed for the A3 was a SPALT (Special Projects alteration!) to provide some protection of the Mk2 guidance system and other electronics from the radiation effects of nuclear detonations, a further 'solution' to the growing perception of an ABM problem. Intended to provide protection from both direct nuclear radiation and EMP (the electromagnetic pulse which can cause widespread damage to electrical equipment, especially from high-altitude detonations) this hardening was introduced in the so-called 'Topsy' improvements.[59]

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57. J. C. Hung and G. B. Doane, III, 'Progress in Strapdown Technology' in Inertial Navigation Components and Systems: AGARD Conference Proceedings No. 116 (papers presented at the 15th meeting of the Guidance and Control Panel of the NATO Advisory Group for Aerospace Research and Development, Florence, 2-5 October 1972), 14-1-14-9, at 14-1.

58. Olson, 4.

59. SASC FY76, Part 10, R&D (Washington, DC: US GPO, 1975), 5353; also Interview.

The A3 was first flight tested on August 7, 1962, had its first submerged launch on October 26, 1963 and became fully deployed on September 28, 1964, when the USS *Daniel Webster* began her first operational patrol.[60] These original A3P missiles were replaced by the Topsy improved A3T version during the late 1960s.[61]

Transit - the Navy Navigation Satellite System

Ironically Project Transit, instigated to provide another source of navigational resets for the FBM submarines, was a direct outcome of the event which stimulated the acceleration of the Polaris programme - the launch of the Soviet Sputnik satellite. Whilst monitoring Sputnik's radio signal, scientists at the Applied Physics Laboratory (APL) of Johns Hopkins University had noted the Doppler effect produced. As with a train whistle the frequency of the signal recorded was higher as the satellite approached the listener and lower as it went away. It was realized that variation in the way the signal changed could be used to precisely plot the orbit taken by the satellite so long as the position of the listener was accurately known. The key insight in the development of Transit then came from Dr. F. T. McClure of APL, who realized that it was possible to reverse the process. If the satellite's orbit was accurately known then the Doppler effect could be used to provide a listener with an accurate positional fix.

This was in March 1958 and by July APL was receiving the first of the funding for the development of a navigation satellite system in Project Transit. This came initially through the Advanced Research Projects Agency and then subsequently directly from SPO. As well as providing navigation fixes and geodetic information for military users, Transit would also become an extremely popular source of navigational fixes for non-military users. In 1981 about 10,000 Transit user sets were in use with about four-fifths owned and operated by non-military users.[62]

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60. Fuhrman, 280.

61. The completion of the A3T exchange in July 1970 is noted in SSPO, 'FBM Facts', 36.

62. Tom Logsdon and Charles W. Helms, 'Comparison Between the Capabilities of the Navstar GPS and other Operational Radionavigation Systems', Paper prepared for Presentation at EASCON '81, Washington, DC (Rockwell International: November 16, 1981), 5.

In May 1960 responsibility for Transit was formally transferred from ARPA to SPO.[63] By then there had been two attempted satellite launches of Transit 1A and 1B. Transit 1A was launched on 17 September 1959 by a Thor Able rocket from Cape Canaveral. However, the third stage rocket failed to ignite and consequently it and the satellite re-entered the atmosphere rather prematurely. Nevertheless, the data obtained during this twenty minute 'partial orbit' confirmed the predictions made for the Doppler tracking concept.[64]

Transit 1B was successfully launched into orbit on 13 April 1960. Amongst other information it provided confirmation of the earth's pear shape and highlighted the inadequacy of current knowledge of the earth's gravitational field for prediction of satellite orbits.[65] Such prediction was, of course, essential for Transit's navigational role by which receivers would determine their own position by monitoring the Doppler shift from a satellite of 'known' orbit. The technique used in Transit required a memory device carried on the satellites which was updated with its predicted orbit from the ground tracking station every twelve hours. The satellite then broadcast this information so that users could obtain their navigation fix.

It was recognized very early in the Transit programme that more accurate geodetic knowledge was required to predict the satellites' orbits with the desired accuracy. Richard Kershner, Director of the Transit programme at APL noted in May 1961 that:

> Meeting the ultimate program goals for Transit thus requires considerable improvement in the present knowledge of these factors (roughly the shape and mass distribution of the earth). This is the primary remaining development challenge of the Transit program.[66]

Thus in the early 1960s accurate geodetic mapping was the primary role of Transit, without which its 'ultimate' goal could not be achieved. Various

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63. Captain Robert F. Freitag, 'Project Transit - Navigation Satellite', US Naval Institute Proceedings (May 1961), 77-83, at 83.
64. Richard B. Kershner, 'Technical Innovations in the APL Space Department', Johns Hopkins APL Technical Digest, Vol. 2 (Jan-March 1981), 264-78, at 265.
65. Ibid, 268.
66. Richard B. Kershner, 'Transit Program Results', Astronautics (May 1961), 113.

other approaches to improving geodetic knowledge were undertaken, such as the Air Force programmes which used flashing-light beacons on satellites and laser-ranging, but Doppler tracking had the great advantage of providing data whatever the time-of-day or weather.[67] This was so successful that:

> By 1964, APL had developed a sophisticated model of the gravitational field of the earth ... that was sufficiently accurate to make possible our goal of better than 0.1 mile navigation at sea. This model was based solely on the analysis of Doppler tracking of a variety of APL satellites. At that point the gravitational-field knowledge was no longer a limiting factor in the navigation accuracy achieved at sea, which instead was dominated by orbit prediction errors caused by the inherent unpredictability of drag and the effect of errors in the estimate of ship's velocity. Continued geodetic work would not contribute to our primary task of providing at-sea position fixes for the Polaris submarines.[68]

By then the first operational navigation satellite (Transit 5BN-2) was in orbit, and further launches during the 1960s established a 'birdcage' usually consisting of five or six satellites in polar orbits. Each of the early satellites varied somewhat from its predecessor as improvements were quickly incorporated. Once a design was settled on RCA was chosen as the prime contractor to build the Oscar series of Transit satellites, the first of which was launched in 1965. When designed the expectations of component reliability led to a prediction of a mean-time-between-failure of two years and it was decided to build twenty Oscar satellites initially.[69] This, however, turned out to be rather pessimistic - the first Oscar satellite lasted thirteen years - and some would still be operational over twenty years later.

Transit thus provided the navigational reset system for the FBM system that it was originally conceived for. However, its initially unintended contribution to geodetic mapping had more profound and widespread applications. It enabled the location of targets to be 'known' with unprecedented accuracy, as could other important navigational sites.

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67. Kershner, 'Technical Innovations', 269; see also Leon H. Dulberger, 'Geodetic Measurements From Space', Space/Aeronautics (June 1965), 34-43.
68. Kershner, 'Technical Innovations', 269.
69. Interview.

According to R. J. Anderle of the Naval Surface Weapons Center at Dahlgren in Virginia: 'Doppler observations of Navy Navigation Satellites have been used by the Department of Defense since 1963 to determine the geodetic positions of isolated sites such as LORAN-C navigation beacons'.[70] Improved gravity mapping could also feed back into gravity models developed for the guidance and navigation systems used in the FBM system.

The Polaris Submarines

Meeting the tight schedule agreed for the interim A1 missile had required the adaptation of attack submarines to produce the *George Washington* class of FBM submarines, SSBN598 to 602. All five were initially equipped with A1 missiles and the first generation of navigation, fire control and launcher systems. Their first overhauls in 1966/67 replaced the A1 missiles with Polaris A3 and upgraded the navigation systems, redesignating the SINS as Mk2 Mod4.[71]

The first FBM submarines designed from the keel up were the *Ethan Allen* class, submarines SSBN608 to 611 and 618. These became operational between June 26, 1962 when the USS *Ethan Allen* first went on patrol, and October 28, 1963. Each was initially deployed carrying A2 missiles with the necessary changes in the launcher systems to accommodate them. Like the 598-class, they carried the Mk-80 fire control system, but in the 608-class this was linked initially to the Sperry Gyroscope Company's Mk3 Mod0 SINS. These were replaced during the late 1960s with Autonetics Mk2 Mod3 SINS.[72]

The remainder of the Polaris submarines were a new, larger type, beginning with the USS *Lafayette* SSBN616, which was launched on May 8, 1962, and began her first operational patrol on January 4, 1964. The

70. R. J. Anderle, 'Error Model for Geodetic Positions Derived from Doppler Satellite Observations', Bull. Geod., Vol. 50 (1976), 43-77, at 43.
71. B. McKelvie and H. Galt, Jr. 'The Evolution of the Ship's Inertial Navigation System for the Fleet Ballistic Missile Program', Navigation: Journal of the Institute of Navigation, Vol. 25 (Fall 1978), 310-322, at 315; 'Navy Standardizes Polaris SINS System', Aviation Week & Space Technology (May 4, 1964), 16.
72. C. D. LaFond, 'New SINS Nears Sea Tests', Missile and Rockets, Vol. 10 (April 30, 1962), 33.

larger size of the 616-class was not the result of any perceived military need; it simply took advantage of the lessons learnt in building the first ten FBM submarines to improve the general design and to provide more space and facilities to make the lives of the submariners more bearable on their long patrols. At the particular behest of Admiral Raborn the submarine design was extended by increasing the length of the centre section.[73] This, however, also increased the displacement of the submarines by about 400 tons over the 608-class, which seemed 'initially a bit of a problem', requiring the addition of an extra 400 tons of lead to provide ballast.[74] But, again, as in the over-design of the missile mount tubes this would eventually prove to be a fortuitous decision. The extra buoyancy provided by the larger submarines meant that they - but not the first ten - would be able to accommodate missiles that were not only larger, but also much heavier, than those of the Polaris generation.

Eight of the *Lafayette* 616-class were initially deployed carrying A2 missiles, with the other one, the USS *Daniel Webster*, and the *James Madison* 627-class and *Benjamin Franklin* 640-class carrying the A3 when it became ready.[75] All carried the new Mk-84 fire control system, with the navigation system depending on which missile was carried. The submarines deployed with A2 had the same Mk2 Mod0 SINS used in the *George Washington* class submarines, whereas submarines carrying the A3 were felt to require greater accuracy. The 627-class were fitted with the Mk2 Mod2 which later had 'field modifications' to convert it to the Mk2 Mod3 installed in the 640-class and retrofitted to the 608-class.[76]

The early Polaris submarines had been authorized piecemeal throughout the last years of the Eisenhower administration, which seemed to have reached no clear decision on how many submarines the FBM force should eventually comprise.[77] By the time J. F. Kennedy

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73. Interview.
74. Ibid.
75. SSPO, 'FBM Facts', 30.
76. McKelvie and Galt, 316.
77. G. B. Kistiakowsky, A Scientist at the White House (Cambridge, Mass.: Harvard University Press, 1976), 162.

became President in January 1961 nineteen submarines had been authorized, with long-lead-time funding for a further five more.[78]

Within the Navy itself a consensus had already been reached that something of the order of 45 submarines was the correct size, providing a neat force structure of five squadrons of nine submarines each. With its self-proclaimed adherence to 'finite deterrence' and internal divisions over what many believed to be the financial burden of Polaris on the surface fleet, a larger force was hard to support. The first public estimate of the Navy's desired number of FBM submarines came from Chief of Naval Operations, Admiral Burke, in January 1957. In response to a congressional request he came up with the figure of 41, admitting later that it took him 'about one hour ... I figured it out on the back of an envelope'.[79] Despite the public counter-city rationale of Polaris the number was apparently based 'entirely on military targets'.[80] During 1958 there were reports of some senators and Navy officers seeking as many as 100 Polaris submarines, but SPO seem to have been quite content with a lower figure. On April 6, 1959 Admiral Raborn proposed a 45 submarine force.[81]

This remained the Navy position throughout 1960 and 1961. Moreover, not only was the Navy reasonably moderate in the size of its demand (as compared to the Air Force's projected ICBM force), but it also preferred to maintain a steady rate of construction at six submarines per year, rejecting additional funds voted by Congress in 1960.[82] When President Kennedy's State of the Union message of January 30, 1961 called for an acceleration in submarine delivery - presumably as a political gesture following his campaigning 'missile gap' rhetoric - the Navy opposed it.

The Navy's projected programme of a 45-submarine force was presented to Secretary of Defense McNamara on July 3, 1961. He

78. See D. Ball, 'Politics and Force Levels', 46.
79. Ibid, 63, fn 7.
80. D. Ball, 'The Counterforce Potential of American SLBM Systems', Journal of Peace Research, Vol. XIV (1977), 23-40, at 25.
81. Ball, 'Politics and Force Levels', 63.
82. Ibid, 64.

responded in September by cutting this by four for a total of 41, but leaving the possibility open of more later. The Navy initially argued for more, even setting a new figure of 50 as a prelude to compromise, but by 1962 had agreed to settle for 41. According to Dr Alain Enthoven, who was closely involved in the relevant decisions, the choice of 41 was:

> **simply an historical accident. There was no precise calculation of the necessary number of missiles. The Administration had inherited a program of 19 [Polaris submarines] then added ten, and then six and six, for forty-one.[83]**

The construction of these 41 submarines over a period of about seven years was a considerable achievement, given the disparate organizations involved. SPO coordinated the activities of BuShips, and its Nuclear Power Directorate headed by Admiral Rickover, and four shipyards: two private companies, Electric Boat and Newport News Shipbuilding and Drydock Company; and two Navy yards, Portsmouth Naval Shipyard and Mare Island Naval Shipyard.

What made this all the more extraordinary was that BuShips and Rickover's Nuclear Power Directorate both had formal control over parts of the programme; BuShips over the submarine construction and Rickover over the nuclear reactor.[84] But the great scope for bureaucratic wranglings and internecine power struggles interfering with the Polaris programme was not exploited.

At least in part this stemmed from a unanimity of purpose that Polaris was necessary, indeed vital, not just for the nation, but also for the Navy. Of course, not everyone shared the latter belief, and some even doubted that the former required such a rate of submarine construction. But such dissent had little power in the relevant organizations at the time, and CNO Burke was very effective in imposing unanimity when required.

Rickover's main concern was to put his nuclear reactor designs to sea. The Nautilis had demonstrated the feasibility of his technical dream, and now only a few years later he had the opportunity for its large-scale

83. Quoted in Ibid, 275.
84. Sapolsky, 69.

application. Without the Polaris programme he would have had to struggle to find such an eager user for what was a relatively expensive technology (at least in terms of initial capital outlay).

Thus although initially concerned that Polaris might divert resources from his reactor work,[85] Rickover soon came to see that their common purpose, to combat Soviet military developments, could also have mutual benefit. Rickover's potential for disruptive interference remained, of course, and was recognized by CNO Burke from the very start. When describing the attributes that led him to choose Raborn to head SPO Burke even went as far to as to emphasize that: 'In other words I didn't want a Rickover in there'.[86] 'Under unwritten orders from Admiral Burke, Raborn and [the Chief of BuShips, Rear Admiral] Mumma excluded Rickover from all the preliminary studies'.[87]

Rickover first received official information on the intended Polaris submarine design on April 16, 1957 when it reached the Nuclear Propulsion Directorate.[88] It was to be a single screw design using the S5W reactor being developed for the *Skipjack* and *Thresher* class submarines. Rickover objected to this, arguing instead that a twin-screw reactor design should be used, but he was overruled, and thus kept out of Polaris design details. In other instances too - for example, when he attempted to restrict the operating depth of the *Ethan Allen* class submarines because he doubted the adequacy of some of their components, or when he was held to be delaying recruitment of FBM submariners with his tortuous interviewing technique (which involved him personally interviewing all officers) - Rickover also found himself forced to concede.[89]

FBM Communications

Communication with the FBM submarines was obviously an important part of the technological system. Even a last resort retaliatory deterrent needs to be told to unleash its vengeance. Indeed the report of

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[85]. Vincent Davis, The Politics of Innovation: Patterns in Navy Cases, the Social Science Foundation and Graduate School of International Studies Monograph Series in World Affairs, IV, 3 (Denver, Colo.: University of Denver Press, 1967), 36.
[86]. Quoted in N. Polmar and T. Allen, Rickover (New York: Simon & Schuster, 1982), 540.
[87]. R. Hewlett and F. Duncan, Nuclear Navy (Chicago: University of Chicago Press 1974), 308.
[88]. Polmar and Allen, 543.
[89]. Ibid, 548-49

the Steering Task Group in the spring of 1957 described command and control communications as a potential 'Achilles heel of the entire Polaris operation'.[90] However, unlike the other FBM technologies, communications was not assigned to a technical branch of SPO.

Instead jurisdiction over FBM communications was shared between SPO, the Bureau of Ships and the Director of Naval Communications.[91] SPO tolerated this arrangement, it seems, for two reasons. Firstly, it was a politic compromise which enrolled other parts of the Navy into supporting Polaris. Certainly, if SPO had attempted to assert dominance over communications it could have involved a damaging dispute. Secondly, although communications were important in the long-term for the operational deployment of Polaris, they were not so vital to the short-term demonstration of its feasibility. SPO were thus not too unhappy to leave those problems to another part of the Navy. Their more pressing concern was to build a missile that could be fired from submerged submarines.

It was also the feeling at SPO that a sophisticated communications system was not essential to the deterrent mission of Polaris. A two part FBM communications programme had been set up, and the first part, aimed at the development of a reliable and secure system, indicated that a basic system was not difficult to achieve, though it would be expensive. The second part of the programme was orientated to long-term research and development of more exotic solutions to the evident weaknesses of the basic approach - mainly that it might take quite a while for all the submarines to receive the message and that you could not be sure that they had.

Earlier submarine voyages -such as the round-the-world trip of the Triton - had already indicated that an antenna raised to the water's surface could receive radio transmissions.[92] By using very low frequency (VLF) radio waves (14 to 30 kilohertz) it was possible to receive messages over great distances. In early 1959 messages sent from a Navy VLF transmitter at Annapolis were said to have been received some six thousand miles

90. Quoted in Sapolsky, 238.
91. Ibid, 238-9.
92. E. Rees, The Seas and the Subs (New York: Duell, Sloan and Pearce, 1961), 169.

away, by a submarine in the Mediterranean.[93] Beginning in the late 1950s six major US VLF transmitters were constructed at Annapolis, Maryland; Cutler, Maine; Oso, Washington; Wahiawa, Hawaii; Yosami, Japan; and North West Cape, Australia.[94] These consist of very large antennas with power outputs of the order of a million watts.[95] Supplemented by a further 21 low frequency (LF) transmitters they have been the main method of communication with the FBM submarines since the first one went on patrol in 1960.

The main problem with VLF and LF is their limited penetration of water. VLF only penetrates to depths of about 9 metres and LF to about 5 metres.[96] To remain contactable submarines must therefore have an aerial continuously deployed near the surface. Two types of aerial can be used for this. One has an antenna buoy at the end of a long cable, which is deployed some 6-9 metres below the surface, whilst the submarine can remain at a depth of about 45 metres. However, submarine speed is limited to about four knots. In the other method a trailing buoyant wire antenna about 550 to 640 metres long is extended behind the submarine which can then travel at around ten knots, but is restricted to a depth of around 15-18 metres.[97]

In addition to these limitations, both forms of antenna also increase the vulnerability of the submarines to detection. The trailing buoyant antenna is also only bi-directional and so limits submarine movement when monitoring a particular VLF transmitter. The buoy antenna was plagued by unreliability problems in its early use by Polaris submarines.[98]

Nor does the VLF system provide complete assurance that messages will be received in a timely fashion, or indeed at all. In 1972 Rear Admiral

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93. Ibid.
94. W. M. Arkin and R. Fieldhouse. 'Nuclear Weapon Command, Control and Communication', in SIPRI Yearbook 1984, 512, fn 156. Following Annapolis, the first to be completed was at Cutler which went on air on January 1, 1961, SSPO, 'FBM Facts', 28.
95. See testimony of Vice Admiral R. Y. Kaufman, SASC Hearings FY1978 (Washington, DC.: US GPO, 1977), 6706.
96. Ibid.
97. M. Spaven, 'Communicating with submarines', Jane's Defence Weekly, Vol. 4 (November 23, 1985), 1152-56, at 1153.
98. Ibid.

Samuel Gravely revealed that 'one of our problems is that some of our messages never get delivered'.[99] For this reason the basic VLF and LF transmitters have now been complemented by other higher frequency systems, in the high (HF) and ultra-high (UHF) bandwidths, able to take advantage of a wide range of US communications systems. These require an antenna to be raised above the water surface, thus risking detection, but provide much greater data transmission rates than the lower frequency systems. In operation FBM submarines would normally monitor VLF, but progressively move to higher frequencies if no messages were available.[100]

Finally, one radio frequency, despite some apparent advantages, has become publicly controversial and had its development delayed for many years. The extremely low frequency (ELF) bandwidth was recognized in the late 1950s as a potential communications system for submarines. Amongst its apparent advantages are much lower attenuation than VLF by the atmosphere (so providing longer range) and by seawater (so penetrating much deeper), and low susceptibility to jamming. The disadvantages are that data rate transmission is very low and that producing the long wavelength requires a correspondingly large transmitter with very high power output.[101]

ELF research began in 1958 and feasibility was demonstrated in 1962, but after a succession of increasingly modest schemes, an operational ELF system has only just been adopted by the US Navy. During this period ELF developments have been canceled once by a US President, once by the Chief of Naval Operations and once by the legal action of the State of Wisconsin.[102] Local environmental opposition to ELF seems to have focused on the potentially harmful effects of the low frequency radiation, though given the comparative absence of opposition to the smaller VLF transmitters, the initial concern may simply have been the sheer size of

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99. Los Angeles Times (May 26, 1972), cited in Desmond Ball, Can Nuclear War Be Controlled (London: International Institute for Strategic Studies, 1981; Adelphi Paper # 169), 24.

100. See B. G. Blair, Strategic Command and Control: Redefining the Nuclear Threat (Washington, DC: Brookings Institution, 1985), 98, fn 35; M. Spaven, Extremely Low Frequency Communications for Submarines: A Background Briefing on British Plans (Brighton: Armament & Disarmament Unit, University of Sussex, January 1986), 4.

101. Spaven, 'Communicating with submarines', 1152.

102. M. Spaven, 'ELF: Surviving the traumas- part 2', Jane's Defence Weekly, Vol 4, (November 30, 1985), 1194-1197, at 1194.

the original ELF proposals. Each successive ELF proposal has been clearly shaped by these concerns as size and power output have been reduced (See Figure 4.1).

Year	Project name	Location	Antenna length	Antenna cables	Power input	Estimated cost
1968-75	Sanguine	Wisconsin	10,000 km	buried	800 MW	$2-300 m
1975-78	Seafarer	Wisconsin	3900 km	buried	20 MW	$590 m
1978-81	Austere ELF	Wisconsin/ Michigan	45 plus 210km	buried	2.4 MW	$455 m
1981-	Project ELF	Wisconsin/ Michigan	45 plus 90 km	above ground	2.6 MW	$260 m?

Figure 4.1: History of ELF Proposals

Source: M. Spaven, 'ELF: Surviving the traumas- part 2', Jane's Defence Weekly, Vol 4, (November 30, 1985), 1194

Moreover, despite its apparent advantages ELF was not a programme that initially found much support in SPO. SPO considered the basic VLF system to be adequately survivable and redundant to ensure retaliation. The great expense and potential local opposition involved in constructing an ELF antenna embracing about 40 per cent of the state of Wisconsin weighed against it.[103]

Other communications developments were also delayed because of doctrinal disagreement within the Navy over the role of the FBM force. Whilst the first part of the FBM communications programme had concentrated on constructing this basic system, the second was devoted 'to seek exotic solutions to weaknesses revealed in the basic systems'.[104] In particular it was desired to develop communications systems which were

103. Eliot Marshall, 'ELF Resurrected After Drowning by Navy', Science, Vol. 212 (May 8, 1981), 644-5, at 644 notes early 1960s ELF scheme involving 6000 mile-long antenna that 'would have embraced 41 percent of the state of Wisconsin'. For environmental case against ELF, see Lowell L.Klessig and Victor L. Strite. The ELF Odyssey: National Security Versus Environmental Protection (Boulder, Colo.: Westview Press, 1980); for Navy rebuttals see, SASC FY 1978, 6677-6756.
104. Sapolsky, 238.

more survivable and which would provide more prompt message transmission.[105]

But in SPO prompt retaliation was not considered necessary for an 'assured destruction' deterrent. The rhetorical question asked was: 'Does the threat of nuclear destruction deteriorate over time?'[106] Others, who favoured a counterforce role for the FBM force, saw prompt response an important feature of communications. A delay in receiving the command to fire might mean the targets (enemy missile silos) would be empty by the time the Polaris missile reached them. Similarly, limited nuclear war, counterforce targeting scenarios (such as briefly advocated by Secretary of Defence McNamara in 1962) required communications systems that would remain operational during a nuclear exchange; they had to be very survivable.

This disagreement over the FBM communications requirements led to a stalemate in the long range programme investigating the development of the more exotic concepts. The cumulative cost of the programme grew whilst little was actually deployed. A 1964 review of the programme was aptly titled 'Where did the $100 million go?'.[107] Eventually FBM communications research was completely separated from SPO in 1967, and a Special Communications Project Office was established.[108] This was directed to provide 'effective communications at all times for the National Command Authorities and Commanders in Chief to the deployed FBM forces ... during and after heavy nuclear and electronic jamming attack'.[109]

Eventually deployed in 1969 was a more survivable system which had been under development since 1962. Known as TACAMO (from 'take charge and move out') this consisted of twelve EC-130 aircraft equipped with VLF transmitters using long trailing antennas. By circling tightly the several mile long antenna is held vertical.[110] In 1972 the Special

105. Ibid, 239.
106. Ibid, 240.
107. Ibid, 239.
108. Ibid, 240; also Blair, 169.
109. Quoted in Blair, 169.
110. See A. B. Carter, 'Communications Technologies and Vulnerabilities' in A. B. Carter, J. D. Steinbruner and C. A. Zraket (eds.), Managing Nuclear Operations (Washington, DC: Brookings Institution, 1987), 217-81, at 237; also D. A. Boutacoff, 'New Tacamo Aircraft

Communications Project Office described TACAMO as 'the only operational survivable element that the Navy has today and most likely will have until the latter part of the 1970s'.[111] Although intended to provide at least one aircraft continuously on patrol in both the Atlantic and Pacific this goal does not seem to have ever been achieved.[112]

Thinking about the Next Generation

Hardly had development of the A3 been approved in September 1960 then consideration of another generation began. Lockheed, the missile contractor, began to push the idea of an A4 missile during 1961, with longer range the main selling point. SPO, apparently, were not immediately enthusiastic, seeing no pressing need for longer range at that time.[113] However, in response to guidance from the Office of the Chief of Naval Operations SPO put forward in November 1963 a proposal for a new missile, by then known as the Polaris B3, which was to carry either three larger warheads or one much larger warhead than A3.[114]

This new missile was to take advantage of increased launch tube size gained by removing some of the shock protection, which had initially been over-designed. This possibility of significant increases in missile size was first pointed up by work done in Westinghouse's systems analysis group, which were tasked by SPO to consider ways of carrying Polaris missiles on surface ships and trucks for the proposed NATO Multilateral Force.[115] Truck mounting, with the missile held horizontal, was the biggest problem. The stowage launch adaptors used in the original Polaris launcher were unsuitable for holding the missile in an horizontal position and the sheer weight of the heavy machined launch tube was considered impracticable.

Being Developed to Support Trident Missile Submarines', Defense Electronics, Vol. 16 (March 1985), 108-11.

111. Quoted in Blair, 170.

112. Blair, 170-71.

113. 'Problems May Cut Polaris A3 Range Goal', Aviation Week & Space Technology, Vol. 75 (September 4, 1961), 31.

114. Interview, ; also D. C. Breasted, 'Navy Seeks Approval For Polaris Follow-on', Missiles and Rockets (November 4, 1963), 18.

115. This section from Interview; also see Robert Lindsey, 'Material Refinement Assists Poseidon Launcher Designers', Technology Week (June 13, 1966), 32-33.

Westinghouse's group came up with a much lighter approach, in which the missile would be enclosed in foam padded resin reinforced fibre glass panels with heavy duty zippers up the sides which would be undone after the missile was loaded into the launch tube. The foam/fibre glass combination supported the missile in all orientations and its resilient flexibility obviated the need for a heavy machined launch tube. The launch tube itself could also be cushioned by foam rather than the liquid springs used in the first Polaris launch system, the Mark 17. The need to cut down on weight also led to consideration of a lighter method of ejection to replace the compressed air system. Two small solid propellant rockets fired sequentially were to provide the pressure required to eject the missile from the launch tube.

Two demonstration trucks were in fact built, but the Multilateral Force never transpired. Westinghouse, however, informed SPO of these launch system advances:

> We pointed out to the Navy that a design could be produced to take existing missiles and develop a launching system with a substantial space saving.[116]

Thus in 1961 SPO had the choice of revising the design of the remaining FBM submarines to make them smaller, or to keep them the same size but squeeze perhaps 20 or 24 missiles into the same size submarines. SPO's first thought, however, was, 'You mean we could put a bigger missile [in the submarines]'.[117]

But with the Polaris A3 development only just started there appeared little immediate justification for another generation of FBM. Instead it was decided to incorporate some of the launcher system improvements into Polaris submarines of the same size and capacity, while retaining the possibility of backfitting a larger diameter missile in the future. The new launch system, known as the Mark 21, was first introduced into the USS James Madison. The major difference was the use of polyurethane foam to replace the 30 or so liquid springs by which

116. Dr George Mechlin of Westinghouse quoted in Ibid, 33.
117. Interview.

the launch tube was suspended in the submarine mount tube. The liquid springs on which the launch tube rested were retained, as were the stowage launch adaptors holding the missile in the launch tube. Also, starting in the USS Nathan Hale, the compressed air ejection system was replaced by one using solid propellant generated steam.[118]

However, the Mark 21 still only accommodated a 54 inch diameter missile. There remained extra space available in the mount tube which meant that the next generation FBM could be bigger, with longer range or greater payload or both. But for the next few years its exact design would be contested, taking even longer to decide than its final name. The only thing that was not in doubt was the missile's eventual development. As Stanley Burriss, general manager of Lockheed Missile and Space Company, put it, 'the question is not "if" it is needed, but "when"'.[119] And, he might have added, "what for".

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118. See R. Lindsey, 'Improved Launch System for Polaris', Missiles and Rockets, Vol. 13 (December 2, 1963), 27-28.
119. Quoted in Robert Lindsey, 'B-3 Polaris Expected To Be Operational In '70', Missiles and Rockets, Vol. 15 (August 24, 1964), 28.

Chapter 5

Poseidon - The 'Interpretative Flexibility' of MIRV

> **...most of us saw the role of Poseidon as not different from the role of its predecessors, namely providing an absolutely dependable, reliable deterrent, and most of us were sceptical about the need to dig out hard targets as an essential element of deterrence. We went along with it to the degree necessary in order to keep the program. The nature of democracy ... is that you're constantly making compromises with conflicting constituencies, and we had to serve the reigning constituency even if we, sometimes we felt they were a little nutty.**
>
> **Admiral Wertheim.**[1]

Polaris A3 was generally seen as a logical extension of the FBM role established by the A1 and A2 missiles - deterrence by the threat of devastating counter-city retaliation. Although some people had favoured more emphasis on enhancing counterforce capabilities, the 'Claw' payload was clearly 'disoptimized' for such a purpose, emphasizing instead ABM penetration. SPO had taken care to exploit the success of the Polaris programme and to emphasis continuity and a moderate approach by retaining the Polaris name for the A3, which was almost entirely a new missile. As thoughts turned to the next generation, again this was initially thought of as another Polaris, be it A4, A3A or, as it was later known, B3.

Lockheed, keen to maintain their workload after A3, were first to suggest another generation of Polaris. Lockheed studies during 1961 suggested that a Polaris A3A or A4 could provide ranges in excess of 3000 nautical miles.[2] But a 1962 Navy proposal to fund development of a Polaris A3A drew the following response from Secretary of Defense McNamara:

> The Navy has proposed the development of a Polaris A-3A missile. The proposed program would have 368 A-3A missiles and 288 A-3 missiles in submarines by FY 1969 at an additional cost of $1.6 billion. The A-3 missile has

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1. Interview.

2. 'Problems May Cut Polaris A3 Range Goal', Aviation Week and Space Technology, Vol. 75 (September 4, 1961), 31.

> approximately 300 lbs. available for decoys; the A-3A has approximately 920 lbs. available for decoys at the same ranges. Although I believe that further development of a more advanced Polaris missile may be desirable, I do not believe that the extra capabilities offered by the A-3 [sic] missile, by comparison with the A-3, are worth the cost of development and procurement. Therefore, I recommend that the Navy proposal be disapproved.[3]

However, by early 1963 it was clear that the new missile could offer 'extra capabilities' by taking advantage of launcher system advances to grow beyond the 54 inch diameter of its predecessors.[4] Accordingly to signify this growth in its dimensions the new missile was now to be known as B3.[5] It still remained unclear, however, whether increased range or payload (or both) was desired.

SPO considered there to be no pressing need for another generation FBM at that time. However, 'in response to guidance and direction from the Navy's planners' SPO proposed a B3 design to DDR&E Harold Brown in November 1963.[6] The new missile was to be 66 inches in diameter and would use this extra capacity primarily to increase the payload rather than the range. SPO Director Rear Admiral Galantin (who succeeded Raborn on February 26, 1962) noted that: 'The range is not as valuable to the submarine system as it is to a fixed system, so it's the payload and how we would slice it...'.[7] The main payload options considered were either a 'Claw' design like A3, but carrying 600 kt warheads or a single large warhead.[8] The larger warheads reflected the desire of some Navy planners to compete with the Air Force by attempting to rival the counterforce role of ICBMs: 'Suffice is to say that they were doing what a military staff would

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3. Draft Presidential Memorandum, Subject: Recommended FY 1964-FY 1968 Strategic Retaliatory Forces (November 21, 1962), 22-23. I am grateful to Lynn Eden for providing me with copies of the now declassified McNamara DPMs.

4. 'Longer Range is Goal of Polaris B3', Aviation Week and Space Technology, Vol. 78 (March 11, 1963), 145.

5. The nomenclature used by SPO came to indicate missile dimension increases alphabetically and range increases numerically.

6. Interview.

7. D. C. Breasted, 'Navy Seeks Approval For Polaris Follow-on', Missiles and Rockets, Vol. 13 (November 4, 1963), 18.

8. Ted Greenwood, Making the MIRV: A Study of Defense Decision Making (Cambridge, Mass: Ballinger, 1975), 33.

be expected to do - go out and clobber the enemy. The enemy being the Air Force!'[9]

But this proposal was turned down by the Office of the Secretary of Defense because the 66-inch diameter missile envisaged was not considered to fully take advantage of the extra launch tube space available.[10] DDR&E Harold Brown told SPO that he did not 'want a still larger missile coming around a few years from now'.[11] He also expressed concern that the new missile should be able to carry 'a payload that was going to be capable of penetrating any anti-ballistic missile system that could possibly threaten the effectiveness of the system in its ability to penetrate to a target'.[12]

This requirement coincided with the view also held by many within SPO of the FBM as an invulnerable, last resort deterrent. But by now there was general agreement that the 'Claw' MRV system used in Polaris A3, which was designed to counter a Nike-Zeus type ABM would have limited effectiveness against the Soviet Galosh. The separation provided was considered to be not large enough to prevent destruction by a single, large exo-atmospheric interceptor detonation.[13] Because of this concern Lockheed had been investigating other approaches that might provide the next Polaris with greater assurance of penetration.

Further development of the A3 approach led to one concept in which multiple re-entry vehicles would be dropped off onto different trajectories calculated to impact in the same target area. To do this a manoeuvring platform was required to release the re-entry vehicles at the desired time and velocity. In this concept, which was known as Antelope, the platform or 'bus' carried out a pre-programmed operation to release the re-entry vehicles with no guidance update to improve accuracy once

9. Interview.

10. This rejection seems to have been informal, as McNamara's December 6, 1963 Draft Memorandum for the President does not mention the proposal at all. A key informal link between SPO and OSD was Captain Robert Wertheim, previously (and latterly) of SPO who served on the staff of DDR&E Harold Brown from 1962 to 1965.

11. Interview.

12. Ibid.

13. Greenwood, 44.

this had begun.[14] Antelope also involved 'decoys, various kinds of false targets and various sources of clutter to ABM radars and it was aimed at penetrating the Moscow ABM system'.[15]

SPO were, of course, already aware of the possibility of such an approach in 1963 when they had proposed the B3 missile, carrying either a single warhead or a 'Claw' design, but with higher yields than A3.[16] But instead of ABM penetration the emphasis of B3 - as intended by its proponents in the Office of the Chief of Naval Operations - was to rival Air Force counterforce capability. Responding to the rejection of this by Harold Brown, SPO set about designing a missile that would accommodate his recommendations, that would gain support within other parts of the Navy, and that would suit their own preferences. Working closely with the relevant staff in Brown's office, it was decided that the preferred design for the next generation FBM should be one that had the flexibility to cope with developments in ABM technology.[17]

Brown had also authorized the Air Force to proceed with development of a new re-entry vehicle in late 1963, with the proviso that it should be a joint Navy-Air Force development. The re-entry vehicle, known as the Mk12, was authorized for development by the Reentry Systems Division of General Electric Company in March 1964.[18] The Mk12 was designed to carry a warhead of about 200 kilotons, allowing several to be carried by the next generation FBM.

However, SPO did not want to use this warhead. A joint development with the Air Force threatened to reduce SPO's control over the programme, and raised the possibility that the resulting technology would not be optimized for their requirements. Ever intent on limiting potential threats to their independence, SPO's leadership seized upon a suggestion made by two members of the Polaris Steering Task Group, Carl Haussmann of Livermore nuclear weapons laboratory and Lloyd Wilson

14. Interview.
15. Ibid.
16. Greenwood, 33.
17. Interview.
18. Robert A. Fuhrman, 'The Fleet Ballistic Missile System; Polaris to Trident', Journal of Spacecraft, Vol. 5, No. 5 (Sept-Oct 1978), 281.

of Lockheed. They proposed using a small warhead design that Livermore had failed to 'sell' to the Air Force in a multiple warhead arrangement that unlike Antelope would allow independent targeting of each warhead.[19]

Such a MIRV (multiple independently-targetable re-entry vehicle) system provided SPO with a design that could allay concern over penetration of ABM systems (which Antelope could do) whilst also providing some enhancement of accuracy (which Antelope could not).[20] It thus satisfied OSD's requirements except in so much as it avoided the use of the Air Force Mk12 re-entry vehicle, and the possibility of extra accuracy made it more appealing to those in the Navy who desired a counterforce FBM.

Indeed within the Office of the Chief of Naval Operations there were some who favoured an explicit shift to a counterforce capability. Rather than combating potential developments in Soviet ABM defences they preferred the new missile to provide the FBM force with a significant hard-target capability and so directly challenge the Air Force's counterforce monopoly. This viewpoint became centred in the ad hoc 'Great Circle Group', headed by Rear Admiral George Miller, which Secretary of the Navy Paul Nitze set up in 1964 as a part of the Navy's Long Range Objectives Group.[21]

Most Navy officers, however, were indifferent to the technical characteristics and strategic mission of the FBM, but highly sceptical of the need for yet another generation so soon after Polaris A3. It was widely believed, and with good reason, that Polaris had been paid for out of Navy funds which otherwise would have gone to the traditional surface fleet.[22]

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19. Greenwood, 45. This small warhead, initially known as the Mark 100, was recommended for Polaris B3 in the June 1964 Scientific Advisory Board Nuclear Panel report called 'Review of Advances in Design of Multiple Warhead Possibilities' and in the August 1965 final report of the Pen-X study. See Ibid, 40.

20. Antelope would, however, continue in a different guise as the UK Chevaline development to enhance Polaris penetration against the Moscow Galosh system.

21. Greenwood, 22.

22. See Harvey M. Sapolsky, The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972), chapter six.

Along with the indecision over its technical specification and strategic rationale, this Navy resistance further delayed the initiation of a new programme. However, the basic concept of the Polaris B3 was decided during 1964. In July 1964, Secretary of the Navy, Paul Nitze, noted 'the fact that the B-3 Polaris missile can give us the option of the same high accuracy that land-based missile systems have.'[23] Although the exact nature of the payload to be carried remained undecided, SPO was officially instructed in November 1964 to proceed with a MIRVed Polaris B3 with guidance improvements to provide greater accuracy.[24]

In December McNamara's draft memorandum for the President noted that:

> I recommend the inclusion in the FY 1966 budget of $35 million to begin development of a new POLARIS B-3. We intend to initiate a project definition for this missile during FY 1965. The B-3 would incorporate improved accuracy and payload flexibility permitting it to attack a single, heavily defended urban/industrial target, or a single hardened point target, or several undefended targets which might be separated by as much as 75 miles.[25]

However, the delay had already led SPO to take an unprecedented step in broadening its scope beyond its single-mission dedication to the FBM system. In June 1964 SPO accepted responsibility for the Deep Submergence Systems Project (DSSP), which was instigated in response to the loss of the nuclear submarine, the *Thresher* in 1963. As Sapolsky has noted this served a dual purpose for SPO:

> Since the technologies involved in deep ocean research were somewhat related to the technologies involved in the FBM system and since some Special Project's Office contractors and part of its technical staff were interested in exploring the opportunities such research presented, DSSP provided both a means to keep the FBM team together until Poseidon was approved and a possible follow-on to strategic missile work.[26]

23. Navy Magazine (July 1964), 28.
24. Sapolsky, 220.
25. Draft Memorandum for the President, Subject: Recommended FY 1966-1970 Programs for Strategic Offensive Forces, Continental Air and Missile Defense Forces, and Civil Defense (December 3, 1964), 30.
26. Sapolsky, 71.

Poseidon C3 - New Name, Same Difference (of opinion)

On January 18, 1965 President Johnson announced the development of the next FBM generation, and gave it special emphasis with a new name. The missile was now to be called Poseidon C3, the change in name apparently inspired by the President's desire to rebuff criticism that his administration was failing to develop new strategic systems. The President's announcement also stated that the new missile would 'double the payload of the ... Polaris A-3. The increased accuracy and flexibility of the Poseidon will permit its use effectively against a broader range of possible targets and give added insurance of penetration of enemy defenses.'[27] The counterforce emphasis was clear as he predicted that 'its effectiveness against a hardened target will be some eight times greater than the latest version of *Polaris*.'[28]

That Poseidon was to be a MIRVed missile had been decided, but this one technology meant different things to different people, both in design and in its implications for nuclear strategy. Whilst its simultaneous development in the Air Force stressed the ability to hit a greater number of widely separated military targets, the concept had developed in the FBM context as a means of defeating Soviet ABM defences. But there too, hard target kill capability was becoming a central, and as it turned out, divisive, issue.

As early as 1962 the Chief of Naval Operations had expressed interest in a hard target capability for the FBM.[29] This was a departure from the Polaris tradition, but SPO was becoming increasingly incorporated into the formal Naval hierarchy. In 1963 SPO ceased to report directly to the Secretary of the Navy, instead coming under the aegis of the Chief of Naval Material, which in turn was subordinated to the Chief of Naval Operations in 1966.[30] Moreover, Defense Secretary

27. Quoted in Greenwood, 6.

28. Quoted in Missiles and Rockets Vol. 17 (October 25, 1965), 16.

29. Fuhrman, 280.

30. Sapolsky, 198.

McNamara's new nuclear strategy - which emphasized the more selective use of counterforce targeting and the initial withholding of attacks on cities - could be taken as implying the desirability of a hard target MIRV for Poseidon.

The hard target issue translated into two tightly related questions: how accurate should Poseidon be, and how large should its warheads be. 'As accurate as possible' and 'big' were the answers given by proponents of a hard-target Poseidon. Ultimately, the dispute came to bear on the issue of accuracy, but at first it was warhead size that was more controversial.

There were three re-entry vehicle candidates for the Poseidon. In addition to the medium yield Mk12, there was another Air Force possibility, the large Mk17 which was under development to enhance the hard target kill capability of the Minuteman ICBM force. SPO's favoured choice was, not surprisingly, a specifically Navy development, the Mk3 re-entry vehicle designed to carry the relatively small (50 kiloton) warhead that the Lawrence Livermore atomic weapons laboratory had proposed to the Polaris Steering Group during 1964.

Initially the Office of the Secretary of Defense (OSD) 'expected to use the Air Force Mark 12 and agreed with some Navy planners that a Mark 17 option should be available to provide greater counterforce capability'.[31] SPO, on the other hand, preferred larger numbers of the smaller Mk 3 as a way of guaranteeing the retaliation mission, even against improved ABM defences, and also to avoid using a re-entry vehicle designed for Air Force use.[32]

At stake in this disagreement within the Navy was not just the virtues of counterforce *per se*, but also the wisdom of a more direct confrontation with the Air Force, rather than a policy of differentiation:

> There were advocates in the Office of the Chief of Naval Operations...who [felt that] anything the Air Force could do the Navy also needed to do.[33]

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31. Greenwood, 45.
32. Ibid.
33. Interview.

This view was centred particularly in the Great Circle Group (in 1967 the Great Circle Group became the Office of Strategic Offensive and Defensive Systems).[34] The Great Circle Group sought a Navy role in a 'warfighting' nuclear strategy and advocated the use of the Mk17 on Poseidon. They were suspicious of SPO. Ted Greenwood reports that 'one Naval officer associated with long-range planning for the Polaris force in the middle 1960s even suggested that SP had made a deal with the Air Force not to try to gain counterforce capability'.[35]

SPO were put under pressure on accuracy as well as on warhead yield. This pressure came not only from Great Circle Group counterforce proponents, but also from the Office of the Secretary of Defense. Secretary McNamara, and his Director of Defense Research and Engineering, Harold Brown, pushed SPO to increase accuracy. Admiral Levering Smith recalled this interchange with Brown:

> ...after agreeing on all the other elements of the system, he told me, 'Well, it is fine, I can agree with these objectives, and I think I can get approval of them, but I cannot get approval of the system unless you put in an accuracy improvement, I cannot sell it to McNamara.'... Mr McNamara's general thought in this regard, as was expressed by Dr Brown to me at the time, was that it would cost very little more to try to improve the accuracy at the same time that you are doing all the rest of the development.[36]

A key adviser to Secretary McNamara recalls that 'it was certainly intended to give Poseidon a significant probability of destroying hard targets'.[37]

This meant improved guidance and more work for the Instrumentation Laboratory, whose sheer size became a matter of concern to the MIT authorities in the 1960s. MIT Vice-President James McCormack investigated whether the Instrumentation Laboratory should be allowed to

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34. Greenwood, 22-23; also D. A. Paolucci, 'The Development of Navy Strategic Offensive and Defensive Systems', United States Naval Institute Proceedings, Vol. 96 (1970), 204-23.
35. Greenwood, 55. A view also expressed by one of my interviewees.
36. SASC FY 1975 , Part 6, Research and Development (Washington, DC: US Government Printing Office, 1974), 3297-8.
37. Letter to author.

take on the Poseidon contract, and how important their contribution would be:

> Apparently, it was in fact Secretary McNamara who insisted on extending the program to incorporate the best that can be had in the guidance system, especially as regards accuracy... Admiral Smith was most informative along the following lines:
> (1) In the Navy's earlier concept, the requirement for a longer-range missile could have been satisfied by a lesser improvement to the guidance system, the development of which would accordingly have most likely been assigned to an industrial contractor, as a logical further development of the Polaris guidance system.
> (2) With Mr McNamara's insistence on getting all of the accuracy possible... the services of the Instrumentation Laboratory definitely came to be required.[38]

Why were SPO not intent 'on getting all the accuracy possible'? First, as with Polaris, Smith and SPO preferred to avoid rigid technical requirements that had to be met come what may, agreeing instead to goals which could, if necessary, be traded off against other system characteristics. Admiral Smith formally could not refuse to accept an accuracy requirement, but by (perhaps realistically) doubling the development cost estimate, he ruled out such a choice. In effect, 'the Technical Director of the Special Projects Office [Levering Smith] agreed to take on the task of providing increased accuracy for Poseidon only if the specific missile accuracy desired were treated as a development goal rather than a development requirement'.[39] To accept a stringent accuracy requirement, and then fail to meet it, would have been very damaging to the reputation of the FBM programme, a reputation carefully built upon the capacity to keep promises.[40]

The second reason for lack of enthusiasm for greatly enhanced accuracy was, again, 'differentiation' - the conviction that the FBM programme was best served by having a mission and identity distinct from

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38. James McCormack, Memorandum to the Files, subject: the Poseidon Contract, 10 November 1968. MIT Archives and Special Collections, Albert Hill papers, 83-40, box 4. Vice Admiral Smith's comment on this in 1986 was that he was sure that he would have said 'that the lesser improvement to the guidance system <u>could</u> have been done by an industrial contractor rather than that it <u>would</u> have been assigned to an industrial contractor' (letter from Vice Admiral Smith, emphasis added.)
39. Sapolsky, 221-22.
40. Interview.

the Air Force programmes, and a desire to avoid 'copycat' competition with these. The third reason was a deep commitment to the retaliatory deterrence, assured destruction strategy. This may originally have been shaped by the organizational logic of differentiation, but it was now deeply felt by many in SPO.

By January 1966 the baseline Poseidon characteristics had been agreed, very much in line with SPO's preferences. The main legitimation for Poseidon was to be possible Soviet developments in ABM defenses and not in ASW so the missile's range was to be about the same as Polaris A3. To counter the ABM 'threat' each missile would carry a large number of the small Mk3 re-entry vehicles and possibly penetration aids of some sort. However, by 'off-loading' some of these warheads it was possible to increase range if Soviet ASW did come to be considered threatening. 'Compatibility' with the Air Force Mk17 re-entry vehicle was still required - the only success of the Great Circle Group's campaign. The immediate question of accuracy, and thus the pressure from the Office of the Secretary of Defense, was resolved by an agreement to aim for about 50% increase over A3,[41] but as a 'goal', not a 'requirement'.

Poseidon Guidance

Initially SPO retained a preference for staying with a modification of Q guidance, even for delivering multiple warheads to different targets. Their view, recalls the then head of FBM guidance at the Instrumentation Laboratory, was that: 'we had a horse that ran right in the proper direction and why change it?'[42] Adapting Q guidance, in a concept referred to as delta Q guidance, required a separate Digital Differential Analyzer for each re-entry vehicle. But with the decision to develop a MIRV system that could deliver many re-entry vehicles over a widely spaced 'footprint', this looked to be a very inelegant solution. It remained under consideration until 1966, when it was decided to go to an explicit guidance formulation.[43] With this the missile constantly 'knows' its position and velocity and

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41. Levering Smith, Robert H. Wertheim, Robert A. Duffy, 'Innovative Engineering in the Trident Missile Development', The Bridge (National Academy of Engineering), Vol 10, No 2 (Summer 1980), 10-19, at 11.
42. Interview.
43. Interview.

recomputes the trajectory required to bring it to its target. Solving the guidance equations used, however, involved 'horrendous ... calculations' computed by iteration.[44] This required more onboard computational capability, which Q guidance had deliberately avoided in Polaris, but by the mid-1960s this was not a limitation. Computational advances also made the use of strapdown guidance appear feasible and it was considered for 'multi manoeuvrable buses' each carrying a strapdown system slaved to a main guidance system in the missile.[45]

This approach was not developed, however, and strapdown was again not considered suitable for the missile guidance. With a manoeuvring bus deploying re-entry vehicles onto various trajectories the amount of reorientation of the system was considered likely to lead to poor accuracy with strapdown.[46] Instead the Poseidon Mk3 guidance system was a traditional stable platform design, an evolutionary development from the Mk2.

With the more complex trajectory flown in a MIRVed system, no one axis would be as dominant as previously, and so the extra difficulties and expense of retaining one PIGA were judged to be not worthwhile. Instead three 16-size (1.6 inch diameter) PIPAs were used together with three 25-size Mod 3 IRIGs. Although similar to their predecessors in basic principle, these devices incorporated 'evolutionary' improvements achieved through continuing work at the Instrumentation Laboratory. In particular the PIPAs were improved by 'orders of magnitude' by reducing bias and scale factor errors as a result of going to permanent magnet torquers, and by changing the method of torquing.[47]

Because of the move to an explicit guidance formulation, the simple Digital Differential Analyzer type of computer was no longer adequate. Instead a general purpose computer was used, drawing on the Instrumentation Laboratory's work on a similar design for the Apollo

44. Ibid.
45. B. O. Olson, 'History of FBM Guidance at CSDL' (10 March 1975, typescript), 4.
46. Interview.
47. Interview.

programme.[48] Its integrated circuits were small scale integration (SSI) with a total gate count of about 5000 (as opposed to 400 for Mk1 and 800 for Mk2).[49] 100K of Read-Only-Memory (ROM) was used for permanent storage of certain programs, such as guidance formulations, and a 12K plated wire Random-Access-Memory (RAM) stored variables that were read in prior to or during flight.[50]

Whereas gravity was implicit in Q guidance, explicit guidance required a gravity model of the earth. A simplified but adequate technique was developed which used a spherical earth, but with offsets precalculated for particular trajectories. Like the Q terms in Mk1 and Mk2 these offsets were carried by the submarine fire control system and read in to the missile computer prior to launch.[51] Also for the first time the general purpose computer provided the capability for in-flight calibration and error compensation. Reflecting the concern about possible ABM developments the Mk3 guidance system was 'hardened' against the effects of radiation.

Stellar Inertial Guidance, Hard Target Kill and the Mk4 Guidance System

Advocates of a hard target kill FBM were not content simply to have Poseidon designed for 'compatibility' with the Mk17 re-entry vehicle. They sought actual deployments of Poseidon equipped with the heavy, counterforce Mk17 as well as the light Mk3. SPO resisted, arguing against a mixed force:

> The Mk17 was going to be expensive, it was going to require a logistical nightmare ... a specially configured missile assigned to special targets as opposed to submarines which could go on patrol with the flexibility to be targeted from one kind of a target to another without having to worry about what you had in the tubes.[52]

48. Graydon M. Wheaton, 'Electronics Manufacturing for Inertial Guidance Systems' (9 May, 1986, typescript), 8. I am grateful to the author for providing me with this.
49. Interview.
50. Wheaton, 5.
51. Interview.
52. Interview.

The argument against the 'mixed force' was not, however, the only resource SPO was able to deploy against the Mk17 re-entry vehicle, for warhead choice began to interact with a crucial new issue in guidance system design. Corporate engineers - outside the existing SPO/Instrumentation Laboratory circle of guidance specialists - began to argue for a radical departure from existing guidance system design. Simple in principle, but with considerable technical and political ramifications, their idea was to supplement the missile's inertial guidance system with information derived from star sightings, taken while the missile was in flight.

This option had not been considered in the early days of US ballistic missile programmes, and in the early 1960s many continued to deem it infeasible - an attitude reinforced by the failure of early tests.[53] Further tests, in the Stellar Acquisition Flight Feasibility (STAFF) programme (which used 'spare' Polaris A1 missiles), were considered successful, but still led to no production contract from the US Air Force, who supported the early stellar-inertial work. The proponents of stellar-inertial, located above all at the Kearfott Division of the General Precision Corporation (now a division of Singer), thus had to turn to the Navy.

Just as the Air Force work was coming to an end, Kearfott engineers came up with a refinement to stellar-inertial guidance that was to be of great significance. It had originally been believed that sightings on two stars were necessary. Kearfott's new argument - known as the 'Unistar principle' - led to the conclusion that one 'optimum' star sighting could give as much relevant information as two. This greatly simplified the mechanical design of a stellar-inertial system at, apparently, little or no cost to accuracy.

What Kearfott could offer the Navy, then, was a technique that promised a considerable increase in missile accuracy. Their argument was that the star sight could drastically reduce the error sources - uncertainty in initial position and azimuth - that had seemed to condemn submarine-

53. A fuller account of the history of stellar-inertial guidance will be found in D. MacKenzie,'Stellar-Inertial Guidance: A Study in the Sociology of Military Technology', forthcoming in Sociology of the Sciences Yearbook 1988 (Dordrecht: Reidel).

launched missiles to be *inherently* less accurate than ones fired from fixed silos on land.

Initially stellar-inertial guidance met with considerable scepticism, even hostility. Armed with this 'Unistar' concept, Marvin Stern, then President of Kearfott (but previously a high level Department of Defense civilian and still 'well connected'), 'went down ... badgered the people in the government and ... sold this concept'.[54] But, despite 'considerable encouragement' from Director of Defense Research and Engineering John Foster,[55] SPO were not enthusiastic.[56]

SPO did not wish to complicate the guidance system to provide extra accuracy which Poseidon's strategic role, in their view a secure counter-city retaliatory deterrent, did not require. What accuracy goals they did have seemed attainable with an all-inertial system. However, the pressure exerted from the Office of the Secretary of Defense eventually told. Also one senior officer, Captain (later Rear Admiral) Robert Wertheim, having been convinced personally, played an important role in SPO's conversion. SPO then pressed the Instrumentation Laboratory to consider it for FBM application.

People there 'were very much opposed to it'.[57] A senior figure in the Instrumentation Laboratory's FBM management recalls the reasons:

> First of all ... there was a certain degree of the 'not invented here' aspect of it. But also there was a concern here at the Lab. that it was an unnecessary complication. ... the guidance system is enough of a pain in its most simple form that you really don't want to complicate it. ... then the other concern that I always had and I still have is that ... there's a possibility of a nuclear explosion in the atmosphere making the stellar system inoperable.[58]

The Instrumentation Laboratory had a great investment in developing more and more accurate inertial components, a line of technological

54. Interview.
55. Interview.
56. Interview.
57. Interview.
58. Ibid.

development which seemed threatened by stellar aided guidance. People there argued that by further refining all-inertial technology one might be able to match the accuracy of stellar inertial without the necessary penalties of weight increase and complication involved in the latter:

> Dr Draper took the gyro out of his pocket and said if you have a good enough gyro you wouldn't need a [star-sensor] ... [Draper and the people at the Instrumentation Lab] didn't think one should gimmick it by adding these crazy things called stellar sensors, that's like putting a band-aid on an inertial system.[59]

One engineer at the Instrumentation Laboratory remembers 'having tunnel vision myself talking with my boss at the time about a star-tracker: "Well, the system has been pretty good so far. We don't need to improve it with a star-tracker, necessarily"'.[60] But eventually the Instrumentation Laboratory was persuaded ('If the guy with the money says he wants it, you convince yourself quite easily that you agree with him'.[61])

But despite their lack of enthusiasm for hard target capability, SPO apparently paradoxically began to highlight this potential attribute of stellar inertial guidance. The reason, it seems, was the continuing battle against the Mk17 re-entry vehicle. In 1966 Lockheed, the missile contractor, did a study for SPO which concluded that with stellar inertial guidance the small Mk3 warhead would have sufficient hard target capability to make the large Mk17 re-entry vehicle unnecessary:

> The Mk17 program ... was made to disappear by the prospect of still further accuracy improvement which made it possible to show that even with the small yield warheads ... you could, potentially at least, threaten damage to moderately hard targets.[62]

Stellar-inertial guidance - even in conceptual form - thus enabled SPO to solve the 'mixed force' problem, by cutting the ground from under the feet of the proponents of the Mk17. It also avoided a substantial loss of market for Lockheed. Like all previous FBM re-entry vehicles, the Mk3 was

59. Interview.
60. Interview.
61. Interview.
62. Interview.

designed and built by Lockheed, while the competitor Mk17 was developed by Avco Corporation.

But having helped 'kill' the Mk17, stellar-inertial guidance was allowed to languish. The lingering resistance at the Instrumentation Laboratory led to various counterproposals over how the concept should be implemented. For example, the Instrumentation Laboratory favoured positioning the stellar sensor telescope on the outside of the Inertial Measurement Unit case, whereas Kearfott had proposed that it be on the stable member:

> ... we had a counterproposal - instead of a telescope that would sit on the stable member inside, we were proposing to add on the outside a case-mounted stellar tracker.[63]

Because of these disagreements development of a stellar-inertial Mk4 alternative guidance system for Poseidon did not really get under way for a couple of years,[64] while the all-inertial Mk3 guidance system proceeded apace.

The plan was to fit the first Poseidon missiles with the Mk3 guidance and later ones with the Mk4. The Mk4 was something of 'a patched-up Mk3 system', with the stellar sensor fitted - as the Draper Laboratory had wanted - on the outside of the IMU case, connected in effect to the outer gimbal.[65] This could take a star sighting following the boost phase (after the two rocket stages had burnt out and separated), prior to deployment of the re-entry vehicles. The image of the star would pass through a telescope and mirror system to the signal plate of a photoelectric 'vidicon' tube. Comparison of the actual star position with that predicted (from a star map) provided the information to correct the guidance system for errors in initial launch position and azimuth knowledge.

Navy testimony to Congress in 1968 clearly refers to the Mk4 development and its apparent intended purpose:

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63. Interview.
64. Interview..
65. Interview.

> During the past year the decision was taken to develop [deleted] to increase the accuracy of Poseidon. When these improvements are completed, Poseidon will be effective both in the assured destruction role and in attacks against hard targets.[66]

Then in January 1969 the new Administration of President Nixon took over. To begin with, that seemed to enhance the Mk4's prospects. Charged with cutting the defence budget, Nixon's Secretary of Defense, Melvin Laird, was looking for cheap ways of toughening up the defense posture. One possibility, suggested to him by stellar-inertial proponent John Brett of Kearfott, was to speed up the development and deployment of the Mk4 guidance system.[67] Laird liked the idea and presented it to Congress in March 1969:

> The increase of $12.4 million for the development of an improved guidance system for the Poseidon missile will advance the initial operating capability (IOC) of that system by about six months. ... This is an important program since it promises to improve significantly the accuracy of the Poseidon missile, thus enhancing its effectiveness against hard targets.[68]

Paradoxically, though, this success was to backfire on the proponents of stellar-inertial guidance. As its political visibility increased, stellar-inertial guidance became openly controversial. The Instrumentation Laboratory's doubts about whether it really was a 'sweet' technology never surfaced in the public domain. But Congressional critics, assuming that stellar-inertial guidance *would* enhance accuracy, began to question whether this was actually desirable.

Key figures in the opposition - in effect, the first Congressional challenge to a 'technical' feature of an FBM system - were Senator Edward W. Brooke of Massachusetts and his aide, Alton Frye. A moderate, black Republican, Brooke had campaigned actively for Richard Nixon in 1968, taking the view that this was the best way to 'maintain influence in a Nixon Administration'. Until 1973, he enjoyed 'cordial access' to the

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66. SASC FY 1969, Part 3, (Washington, DC: US GPO, 1968), 1052.
67. Interview.
68. Quoted in Stockholm International peace Research Institute, SIPRI Yearbook of World Armaments and Disarmament, 1968/69 (Stockholm: Almqvist & Wiksell, 1970), 109.

President.[69] Both through his personal rapport with Nixon, and in the Senate, Brooke campaigned against hard-target kill capability.

In the climate of the time, in which the public rationale for US possession of nuclear weapons was dominated by the ideas of mutually assured destruction, this forced the Administration onto the defensive. Thus a lengthy letter from Brooke to Nixon on 5 December 1969, seeking reassurances 'that the United States will not seek a capability to disarm the Soviet Union', led, after consultations between the White House and Department of Defense, to a reply from Nixon on 29 December asserting: 'There is no current US program to develop a so-called 'hard-target MIRV capability'.[70] Reality was then brought into line with this assertion by cancelling the Mk4 stellar-inertial guidance system, and the following summer John Foster, still Director of Defense Research and Engineering in the new Administration, probably referred to this when he testified that:

> We had a program of investigation along these lines and last year I canceled it. My purpose was to make it absolutely clear to the Congress and hopefully to the Soviet Union, that it is not the policy of the United States to deny the Soviet Union their deterrent capability.[71]

The ease with which this Congressional pressure ended the Mk4 programme reflects, as Ted Greenwood notes, the ambivalence about it in SPO.[72] Foster, too, recalls that he personally was influenced by the argument of SPO Director Levering Smith that too much accuracy in the FBM force was strategically destabilizing.[73] By contrast, the Air Force's Advanced Ballistic Reentry Systems (ABRES) programme also had funding for hard-target aspects of it curtailed at the same time, but the Air Force easily circumvented this restriction.[74] SPO seemed to take almost the opposite view, that the Mk4 was a non-essential programme which provided them with some buffer funds which could be redirected to more

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69. A. Frye, A Responsible Congress: The Politics of National Security (New York: McGraw-Hill, 1975), 55.
70. Ibid, 69-70.
71. Ibid, 70, footnote 1.
72. Greenwood, 136.
73. Interview.
74. Greenwood, 137.

critical areas if required.[75] Current Poseidon missiles still bear testimony to the seriousness with which the Mk4 star sensor was considered (a 'trap door' through which the star sighting was to be taken remains). However, SPO's approach to it was pragmatic and contingent. If the Office of the Secretary of Defense wanted stellar-inertial guidance (and was prepared to pay for it), that was fine, especially as it helped to bury the Mk17 re-entry vehicle. If then enthusiasm and funding for the Mk4 guidance system dried up, that was fine too. SPO's programme managers had quite enough to do meeting their goals with Poseidon as it was. Technologists intrigued by the new challenge of stellar-inertial guidance, advocates of a hard-target FBM, and, of course, Kearfott would, however, all have another chance with the next generation FBM, which was already on the horizon.

Developments in Navigation Technology: The Transit Improvement Program and the Electrostatically Supported Gyroscope

However, while the Mk4 stellar inertial guidance system programme was canceled in 1968, another programme was being initiated to provide improvements in FBM submarine navigation. But, in contrast to the hard-target rationale of the Mk4, the Improved Navigation Program was justified on the grounds of increasing navigation reset intervals and availability, and hence submarine survivability, and so did not excite Congressional opposition to accuracy improvements.

The Improved Navigation Program covered a variety of developments aimed at enhancing various aspects of FBM navigation. These included the development of the 'phase-shift' Loran-C transmission method whereby on-board cesium-beam atomic clocks allow the synchronization necessary 'to provide range-range operation from two stations in addition to the normal three-station hyperbolic time-difference mode'.[76] When it became operational in 1974 this provided a large increase in the geographic availability of Loran-C navigation resets.

75. Interview.

76. King T. A. and H, Strell. 'Underwater Navigation' entry in McGraw-Hill Encyclopedia of Science and Technology (New York: McGraw-Hill Publishing Company, 1982), 399-402, at 400.

Another major development was a new generation of Transit satellites known as TIPS (Transit Improvement Program Satellites). The first experimental improved satellite (known as TRIAD or TIP-1) was launched in September 1972.[77] The improvements were touted as providing greater radiation protection and longer useful life in the event of the loss of ground station control.[78] The major innovation was a drag compensation system (known as DISCOS, for Disturbance Compensation System), which 'is a device that compensates for the effects of aerodynamic drag forces and solar radiation pressure which act on the satellite in orbit, thus permitting the satellite to follow an orbit influenced solely by the gravitational field of the earth'.[79] The concept used is theoretically quite simple, and had been known for many years. A proof mass unsupported within the DISCOS unit is shielded by the unit from atmospheric drag and solar radiation and so experiences only gravitational forces - it follows a purely gravitational orbit. The DISCOS control system senses the motion of the proof mass relative to itself and responds to maintain their separation using Freon 14 cold gas thrusters - thus allowing the satellite to emulate the gravitational orbit of the proof mass.

DISCOS, along with an onboard general purpose computer to compensate for predictable drift in the satellite's reference oscillator, provides the capability to maintain accurate navigational broadcasts for over a week, as compared with the previous Oscar satellites which require orbital determination updates every day or so.[80] It may also provide somewhat more accurate navigational fixes as compared to the best offered by prior Transit satellites, but the primary rationale seemed to be extension of accuracy following loss of ground stations.

However, although TIP satellites were tested during the 1970s, the first of the production 'Nova' satellites was not launched until 1981.[81] Other Transit improvements were introduced in 1975 when the gravity

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77. SASC FY1975, Part 6, (Washington, DC: US GPO, 1974), 3280.
78. Ibid, 3278.
79. J. Dassoulas, 'The Triad Spacecraft', APL Technical Digest, Vol. 12, No. 2 (April-June 1973), 2-13, at 2.
80. T. A. Stansell, Jr., 'The Many Faces of Transit', Navigation: Journal of The Institute of Navigation, Vol. 25, No. 1 (Spring 1978), 55-70, at 62.
81. Owen Wilkes and Nils Petter Gleditsch, Loran-C and Omega: A study of the military importance of radio navigation aids (Oslo: Norwegian University Press, 1987), 99.

model used for orbit determination was changed (from the one originally developed by the Applied Physics Laboratory to the new World Geodetic System 1972[82]) providing greater accuracy,[83] and when a technique was introduced to allow reduced exposure of the FBM submarine's BRN-3 antenna for navigational fixes.[84]

However, another of the developments which came to be funded under the Improved Navigation Program, the electrostatically suspended gyroscope (ESG), would, like the TIP satellites, only find operational deployment in the 1980s. The ESG was in many ways directly analogous to the stellar inertial guidance system. But while decisions about Poseidon guidance became explicitly political, decisions about SINS technology remained firmly 'inside the black box', treated as merely technical. Yet the nature of developments in guidance and navigation technology were remarkably similar. There too, evolutionary improvement of existing technology was challenged by a radically different technology whose proponents promised greatly enhanced accuracy. There too, these proponents came from outside the traditional circle of suppliers to the FBM programme. There too, the challenge failed, at least for the time being.

The challenge was right to the core of existing SINS technology: the gyroscopes. Because they had to keep the SINS stable platform in accurately known orientation for far longer than did missile guidance gyroscopes, these were crucial. Their design had stabilized to a 'paradigm' involving both flotation of the can containing the rotor in fluid and self-activating gas bearings for the rotor to spin on. The challenge involved doing away with conventional bearings altogether. It emerged from work done in the early 1950s by Professor Arnold Nordsieck of the University of Illinois. Nordsieck sought to construct the 'ultimate gyroscope' by supporting the gyro rotor in a vacuum in an electrostatic field.[85]

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82. On WGS 72, see T. O. Seppelin, 'The Department of Defense World Geodetic System 1972', The Canadian Surveyor, Vol. 28, No. 5 (December 1974), 496-506.
83. Stansell, 'Many Faces', 62.
84. SASC FY 1975, 3286.
85. H. W. Knoebel, 'The Electric Vacuum Gyro: Pinpoint for Polaris Launching', Control Engineering, (February 1964), 70-73.

In the mid- to late-1950s, SPO supported exploratory studies of Nordsieck's concept at Honeywell and General Electric, 'with a view to the possible use of ESGs in Polaris submarines'.[86] The potential advantages were clear. The electrostatically suspended gyroscope was canvassed in the early 1960s as having drift rates of the order of 0.0001 degrees per hour.[87] In that period, a gyro with 0.01 degrees per hour drift was considered good, and though SINS gyros would certainly have been considerably better than that, the ESG could be put forward as a major possible improvement.

What was at stake was not simply the technology at the core of the SINS, but the organization that would supply it. Although Autonetics, which was consolidating its position as the sole SINS supplier, had begun studying the ESG in 1959,[88] the early running on the technology was made by Honeywell. Though Honeywell had and has an involvement in the manufacture of inertial components for ballistic missile guidance systems, it was an outsider to the SINS programme. As a supplier of inertial components, but not systems, Honeywell seems to have been concerned to enlarge its involvement through developing innovative technologies.[89]

But despite the promise of greater accuracy, the ESG 'for years seemed destined to remain only a cumbersome laboratory curiosity'.[90] Simple and elegant in concept, actually producing ESGs in any quantity proved to be extremely difficult. A completely spherical ball is best for purposes of suspension, but difficult to make. The Honeywell ball had to be machined to within 5 millionths of an inch, and 'during fabrication the hollow sphere is formed with a slight elongation along its spin axis such that it will become perfectly spherical when rotating at high speed'.[91]

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86. P. J. Klass, 'New Gyro nears Operational Use', Aviation Week and Space Technology, (19 June 1972), 50-52.
87. P. J. Klass, 'Navy to Test Electrically Suspended Gyro', Aviation Week and Space Technology, (6 February 1961), 85-91.
88. B. McKelvie and H. Galt, Jr. 'The Evolution of the Ship's Inertial Navigation System for the Fleet Ballistic Missile Program', Navigation: Journal of the Institute of Navigation, Vol. 25 (Fall 1978), 321.
89. In addition to pushing the ESG, Honeywell also played the key role in the development of the laser gyroscope, a technology that since the late 1970s has brought the corporation a major share of the aircraft inertial navigation market.
90. Klass, 'New Gyro', 50.
91. Klass, 'Navy to Test', 87; P. J. Klass, 'Inertial System uses Electrostatic Gyros', Aviation Week and Space Technology, 30 September 1963, 87-89, at 88..

Sphericity was not the only problem. Without physical contact between ball and case, reading out the ball's orientation (which was the point of the whole exercise) was tricky. The Honeywell gyro, and a research gyro developed at the University of Illinois in the early 1960s, used optical sensors to track a special pattern on the surface of the ball.[92] And of course there was the fear of what would happen if there was an interruption of power supplies when the ball was spinning. Without the supporting electrostatic forces,the ball would 'crash' and disintegrate. Because the gap between the ball and the walls of the cavity in which it was spun was tiny (of the order of a hundredth of an inch), sudden shock or vibration could also cause a catastrophic 'touch down'.

Honeywell never succeeded in getting its ESG adopted by the Navy. Although advanced in the early 1960s as 'pinpoint for Polaris launching', neither the Polaris nor Poseidon programmes made use of it. This was not because it failed to meet accuracy goals in performance terms. One report from the time noted that:

> Such [electrostatically suspended] gyros manufactured by Honeywell have been undergoing tests aboard the USS Compass Island for several years with very gratifying results. Performance specifications have been exceeded ...[93]

However, the ESG had to compete with the evolutionary improvements of the more familiar SINS technology.[94] SPO's judgement at the time was that the ESG performance was 'modestly better' than the projected

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[92]. Klass, 'Navy to Test', 87; Knoebal, 71.

[93]. R. C. Langford, 'Unconventional Inertial Sensors', paper presented to Second American Institute of Aeronautics and Astronautics Annual Meeting, San Francisco, 26-29 July 1965, 18.

[94]. The original Mk 2 Mod 0 SINS was improved gradually through a series of changes, such as a better 'binnacle' to enclose it which reduced errors due to variations in temperature and air flow. After these modifications the Mk 2 Mod 0 was renamed the Mk 2 Mod 4, providing good enough performance for the 2500-mile Polaris A3. Autonetics new SINS, the Mk 2 Mod 2, featured an improved version of the G7A gyro, the G7B, and was installed in submarines twenty to thirty (the 627-class). This was designed to allow a modification involving the addition of a fourth monitor gyro which provides compensation for the drift rates in the other gyros. Thus modified it was known as the Mk 2 Mod 3 and in this form it was retrofitted to the five 608-class submarines (formerly using the Sperry Mk 3 Mod 0) and fitted to the final twelve FBM submarines (the 640-class). By the end of the 1960s the 41 Polaris submarines contained a mixture of Mk 2 Mod4 and Mk 2 Mod 3 SINS. A further modification of the Mk 2 SINS, the Mod 6, was developed and retrofitted into the 31 submarines which were converted to carry Poseidon. See McKelvie and Galt.

performance of improved SINS. But it 'would cost a lot of bucks' to get that modest improvement.[95] As Poseidon was retrofitted to the FBM submarines various improvements were made to the navigation systems, but conventional SINS remained at their heart.

The argument against the ESG was thus not dissimilar to the argument against stellar-inertial guidance. The proponents of the ESG, however, do not seem to have been effective 'heterogeneous engineers' like the proponents of stellar-inertial guidance. Here there was, it seems, no lobbying of high officials[96], no engineer feeling the need to 'join the power structure' (ie the Department of Defense) to secure the technology's acceptance, as one key corporate proponent of stellar-inertial guidance decided was necessary as a result of the failure to get it incorporated in Poseidon. Quite possibly as a result, the advocates of hard target kill did not seize on the ESG to push for its early deployment, and Congressional doves were never caused to oppose it. It remained a 'technical technology', not a 'political' one.

But it did not die either. Like stellar-inertial guidance, the ESG was to find success. Interestingly, though, it was to find success in a different design, and produced by a different corporation. Honeywell, its key proponent, never secured a place in submarine navigation.

Building Poseidon

The debate over the technical characteristics of Poseidon was centred on those technologies that were seen as defining its operational capability. Whether it would continue to be seen as an extension of the counter-city role attributed to Polaris, or, as some desired, as a counterforce system came to rest on the twin attributes of warhead size and system accuracy that traditionally define 'hard target kill capability'. But of the principle technologies that determine accuracy - guidance and navigation - only one became openly controversial. Guidance improvements were publicly touted as a means of enhancing hard target kill and thus attracted

95. Interview.

96. There was lobbying of top Air Force officials to secure the ESG's incorporation in a new navigator for the B-52 bomber (Interview), but not, as far as we know, of Navy officials.

criticism. Navigation developments, on the other hand, did not attract the advocacy of counterforce proponents and continued to be justified on the widely favoured grounds of improving submarine survivability.

Other subsystems of Poseidon were less contested. The payload was to be delivered to a range that was nominally the same 2500 nautical miles as Polaris A3, depending on how many warheads were carried. At the maximum loading of fourteen warheads the range was about 1800 miles[97], but by offloading this could be increased to almost 4000 miles. Although some advances were made in structural weight savings and improved propellent performance, much of the increased payload capability stemmed simply from Poseidon's larger size. As in Polaris A3, Poseidon two stages both use fibre glass chambers with the first-stage propellent a composite type, and that of the second stage double base.

Compared to A3, the Poseidon propulsion development was considered a conservative technological step.[98] In the only significant compliance by SPO with the new defence procurement regulations developed under McNamara, the Poseidon propulsion became the first major FBM subsystem to be competitively tendered.[99] Hercules Powder Co. collaborated with Thiokol Chemical Corp. in a 'joint venture' to produce both stages, with Hercules responsible for the entire second stage and the fibre glass casing for the first stage into which Thiokol loaded their composite propellent. Aerojet, who had built Polaris propulsion systems, also tendered for both stages, but were squeezed out on cost.

Development of Poseidon propulsion proceeded without any serious problems, but, later, after deployment began to encounter unexpected failures. After several years' investigation this was finally identified as due to age-related cracking of insulator rubber. The transition from the storage state of the missile in the launch tube to the high pressure following ignition led to failures which were found to be related to the missile storage temperature. This was largely eliminated by increasing the missile launch tube temperature, thus stopping the rubber

97. SASC FY 1977, (Washington, DC: US GPO, 1976), 6553.
98. Fuhrman, 281.
99. 'Hercules, Thiokol Win Poseidon Work', Missiles and Rockets (October 25, 1965), 16; also Sapolsky, 207.

insulators from becoming so brittle. What made this failure particularly intriguing, however, was that SPO had two manufacturers of the insulators, which provided markedly different failure rates:

> Both of them made these insulators to the 'identical process' - I use that in quotations - best we could tell they were identical, everything we specified they were identical. Obviously we didn't specify enough. The problem developed in the one insulator fairly early in its life and eventually developed in the other one, but a lot, lot later. And we never could figure out ... what was different about the two processes as they actually did it that created this problem.[100]

Thrust vector control in both stages is provided by single nozzles, which are controlled by gas generators. With the introduction of the MIRV bus for deployment of the re-entry vehicles, second stage thrust termination was again considered necessary. Instead of the prebuilt plugs used in Polaris A1 and A2 the thrust termination vents in Poseidon were simply blown out pyrotechnically through the homogeneous fibre glass chamber.

This thrust termination is intended to leave the 'bus' traveling at the desired velocity for it to release the individual re-entry vehicles onto trajectories that will take them to their intended targets. To reorient the bus so as to drop re-entry vehicles onto different trajectories requires a propulsion system of some sort. Whereas the Air Force Minuteman III MIRV system uses liquid fuel rockets for this purpose, the Navy preferred to avoid liquid propellents for submarine based systems. Instead they choose to use solid propellents, which are considered safer, but more difficult to mechanize. Whereas liquid rockets can be turned on and off and throttled precisely, solids burn at a constant rate once lit and cannot be easily stopped and started.

The design of the bus for Poseidon thus required a complicated system of valves to provide the desired manoeuvring during the deployment phase, whilst the solid rocket system burnt continuously. This manoeuvring and the release of re-entry vehicles is controlled by the guidance computer, through a steering implementation developed by

[100]. Interview.

close collaboration (compared to the previous friction) between Lockheed and the Draper Laboratory.

One unexpected outcome of using a manoeuvring bus which experienced changes in the direction of acceleration was that loose particles moving around in the electronics produced excessive unreliability.[101] Such a phenomenon had not been apparent under the linear acceleration that Polaris electronics components were subjected to.

Development of the re-entry vehicles which the bus carried was even more fraught with difficulties. Like the Polaris Mk1 re-entry vehicle the Poseidon Mk3 was a heatsink design made of beryllium. In addition it incorporated an ablative graphite nosetip and special outer coating intended to provide some protection from X-ray deposition caused by ABM nuclear detonations.[102]

The initial concern during development was ensuring that the re-entry vehicle design would not suffer from the phenomena known as spin-up and spin-down. Reentry vehicles are spun so that the effects of atmospheric re-entry are symmetrical, and to limit loss of accuracy. Spinning is particularly important in ablative designs where uneven ablation would alter the aerodynamic characteristics of the re-entry vehicle and so severely reduce its accuracy.

The Poseidon Mk3 is a very small re-entry vehicle and the asymmetries that occur as it deforms during re-entry are especially significant.[103] In its development phase asymmetry-induced torques resulted in the occurrence of spin-up and spin-down. As one Lockheed manager put it: 'It would take a banana shape and that would cause a trim ...[and it] would end up either rolling up or down'.[104] This was a serious concern as spin-up can lead to the destruction of the re-entry vehicle and spin-down through zero greatly reduces system accuracy.

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101. Interview.
102. Interview.
103. For a discussion of the asymmetries which result in re-entry vehicle torques, see, R.W. Carlson and C.A. Louis III, Introduction to Re-entry Flight Dynamics , LMSC-D050690 (Sunnyvale, Calif.: Lockheed Missiles & Space Company, 15 March 1968),
104. Interview.

With this problem solved there was still a further upset much later in the program. The re-entry vehicle carbon nose tips had worked well during prototype flight testing, but when the program moved into its production phase apparently identical nose tips started to breakup in flight tests, especially those conducted over longer ranges:

> These graphite nosetips had worked perfectly up throughout the development of Poseidon without exception. We never had a failure in a development flight test and we went into production, we started our testing and ... lo and behold we started getting failures. A few, not large numbers, but a small percentage of the nosetips of the re-entry vehicles broke up in flight. And this presented a tremendous challenge to us because what in the world was going on. Something had changed and we examined the graphite with great care using every non-destructive test means we could to find out what was different about the graphite that had gone into the production nosetips and the ones that we had used in the development flight testing. We could find nothing other than the fact that they were produced in different facilities. It turned out that the manufacturer, who was Union Carbide, had used an R&D facility, a small furnace, for graphitizing the nosetips in the processing and when we'd gone into production, very much larger numbers, they had shifted to their production facility and we concluded that the control of the thermal gradients and the temperatures in the large production furnaces was just not, could not have been the same. We couldn't find any other reason for this statistical variation in quality.[105]

In 1973 a three-year modification programme was instigated to remedy the design and replace already deployed re-entry vehicles.[106]

Deploying Poseidon

Despite the development problems Poseidon was deployed only two months behind schedule. At the end of March 1971 the USS *James Madison* went on patrol, her Polaris A3 missiles replaced by Poseidon during her first overhaul. Conversion of Polaris submarines to carry

105. Interview.

106. See 'The Poseidon Misadventure', Business Week (October 13, 1973); 'Gradual Poseidon Modification Planned', Aviation Week & Space Technology Vol. 99 (September 17, 1973), 19.

Poseidon mainly involved changes in the navigation, fire control and launcher subsystems.

The submarine SINS were improved to provide better performance with the Mk2 Mod3 upgraded to a Mk2 Mod6 configuration. This used a redesigned Inductosyn package for heading readout so as to reduce the transmission error, and the SINS G7B gyroscopes were selected to a higher standard:

> G7B gyros were screened during factory selloff tests. These tests included the final drift test, self-induced vibration test, and output axis hysteresis test. If the gyro met specified performance criteria for these tests, it was designated a C-3 gyro for use on the MK 2 MOD 6 SINS. If the gyro exceeded the C-3 criteria, but met a less stringent set of criteria, it was designated an A-3 gyro for use on the SINS installed in the submarines still carrying Polaris A-3 missiles.[107]

Other new hardware included a considerably more powerful computer based on the Univac CP-890 to replace the NAVDAC, and a new Loran-C receiver, the AN/BRN-5 to replace the original AN/WPN-3.[108] New calibration techniques were also introduced for the Poseidon navigation system.[109] The Mk-88 fire control system used in the submarines that were converted to carry Poseidon was simply an evolutionary improvement of the MK-84 used for Polaris A3.

In the launcher system the developments that had originally 'discovered' the extra tube space that made Poseidon possible were taken advantage of. The Mark 21 launch system originally installed in the submarines to hold Polaris A3 missiles had been designed so that modular replacement allowed for relatively simple upgrading to the Mark 24 system for Poseidon. The heavy launch tube and stowage adaptors used for Polaris were replaced with a thin launch tube taking up almost all the

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107. McKelvie and Galt, 317.
108. See W. N. Dean and D. P. Roth, 'The AN/BRN-5 Loran receiver', Navigation - Journal of the Institute of Navigation, Vol. 23 (Winter 1976), 287-97.
109. McKelvie and Galt, 317-18.

space in the submarine's mount tube. Foam padding replaced the stowage adaptors in holding the missile snug in the launch tube.[110]

Conflicting Interests: 'Follow-on' and Counterforce

Poseidon was the outcome of a number of influences, some of which were conflicting. SPO as an organization needed a new missile development to justify its special status. With the Polaris A3 development due to finish in 1964, there was a danger that a delay in starting development of another FBM generation would at best lead to the break up of the expertise that SPO had assembled (both internally and in its contractors), and at worst might lead to the dissolution of SPO.

But Poseidon cannot be simply explained as the inevitable product of a follow-on imperative.[111] SPO and Lockheed certainly were keen that there should not be too long a gap between FBM generations, but other aspects of the FBM programme, such as maintenance of existing systems, were both considered important at SPO and a source of work for Lockheed. Moreover, whatever the incentives pushing organizations to ensure rapid 'follow-on', they still need to be sanctioned by Congress and the Department of Defense. In the case of Poseidon, neither of these were to simply present SPO with a 'blank cheque' to build what it wanted. Indeed Poseidon was the first FBM system to be consciously subjected to a Congressional cut when the Senate Defense Appropriations Subcommittee refused to allow more than two Polaris submarines to be converted to accept it.[112] The Office of the Secretary of Defense (OSD) was supportive of a new generation, but wanted to ensure that the next FBM would provide some extra capability. As it was, the delay in authorization of Poseidon which stemmed from OSD's directed redesign led to SPO taking on the Deep Submergence Systems Project as 'a means to keep the FBM team together until Poseidon was approved'.[113]

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110. See Robert Lindsey, 'Material Refinement Assists Poseidon Launcher Designers', Technology Week (June 13, 1966), 32-33.
111. See James R. Kurth, 'Aerospace Production Lines and American Defense Spending', in Steven Rosen (ed.), Testing the Theory of the Military-Industrial Complex (Lexington, Mass.: Lexington Books, 1973), 135-56.
112. Sapolsky, 225.
113. Ibid, 71.

In particular, SPO found the rest of the Navy very unresponsive to the idea of replacing Polaris with another generation missile. The costly Polaris programme was widely perceived in the Navy as having used funds that would otherwise have gone on more traditional missions. To many these missions seemed rather more pressing than the development of another generation FBM. Such resistance was only overcome during 1964 when Secretary of the Navy, Paul Nitze persuaded CNO Admiral McDonald that the Poseidon programme should go ahead.[114]

The follow-on to Polaris was, then, neither easy nor particularly swift. Even after a decision to proceed was made, the actual design remained contested for several years. Many who were supportive of a new missile, particularly those in the Great Circle Group, continued to press for technical characteristics orientated towards counterforce. More importantly counterforce was also seen as desirable by McNamara's OSD and accuracy improvements were sought in the FBM for this purpose, as for some time, was the possibility of carrying larger warheads.

However, although SPO's autonomy was cut back during the 1960s skilful heterogeneous engineering maintained the programme's continuity without significant compromises. Accuracy remained only a goal, and not a requirement. Stellar-inertial guidance was used as a argument against the Mk17 heavy warhead, and then itself dropped. In its final form Poseidon's technology reflected more SPO's concern of ABM penetration than that of those who desired to rival the counterforce role of the Air Force. SPO's leadership thus maintained the aspect of the FBM programme it considered most important, its ability to meet the promised goals - particularly that of an assured retaliatory deterrent - and therefore its differentiated role as compared to the Air Force.

The outcome was a weapon system whose accuracy/yield combination was considered inadequate for the destruction of the hardened targets, such as missile silos, which began to appear in the Soviet Union in 1964. But although it never achieved the counterforce capability that some had desired for it, Poseidon provided more such capability than Polaris. As well as the traditional FBM targets - large, vulnerable

114. Greenwood, 44.

urban/industrial areas - Poseidon could now also be directed against a range of smaller, soft targets, including non-strategic military installations, R&D centres and other important industrial facilities away from cities. Indeed the legacy of a forty-one boat fleet, combined with the decisions over Poseidon's payload, almost demanded such targets be included in the SIOP. With all but the first ten FBM submarines to be converted to Poseidon, the increase in targeting depended on how many warheads each missile carried. To provide extra range Poseidon was initially deployed carrying considerably less than the maximum fourteen warheads per missile allowed in SALT - perhaps as few as six per missile - giving a sixfold increase in targets as compared to the Polaris missiles replaced.[115] Some extra could go to providing additional assured destruction - just in case of Soviet ABM deployments - but lacking effectiveness against hard targets, the rest provided a capability which 'required' the addition of many marginal targets to the SIOP.[116]

Poseidon, thus, left a contradictory legacy. Although SPO's autonomy was much reduced during the 1960s, it nevertheless managed to avoid serious setbacks to the programme. But Poseidon did leave SPO's image of technical and managerial competence tarnished. The long, drawn-out development period, the demanding nature of the MIRV technology, and increasing limitations on SPO's independence all served to highlight difficulties. But although there were serious problems which continued to impair reliability even after Poseidon became officially operational, these were not unprecedented in the FBM programme. SPO and its contractors (which remained virtually the same from Polaris to Poseidon) did not suddenly become poor technologists. What changed, most crucially, was the world in which they operated. Unlike with Polaris, SPO could no longer command unlimited funding whilst remaining able to shift performance goals to meet achievement. SPO became subject to increasing interference from the Navy's leadership, from the Office of the Secretary of Defense and from Congressional scrutiny. The special powers

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115. See 'Poseidon Missiles To Get More RVs - But Not 14', Aerospace Daily (October 30, 1980), 331-32.

116. See D. Ball, 'The Role of Strategic Concepts and Doctrine in US Strategic Nuclear Force Development' in B. Brodie, M. D. Intriligator and R. Kolkowicz (eds.), National Security and International Security (Cambridge, MA: Oelgeschlager, Gunn & Hain, 1983), 37-63, at 55.

granted by Burke to Raborn for Polaris dwindled,[117] as SPO no longer came to be considered 'special' - indeed its name was changed to Strategic Systems Project Office in 1967.

Developed in the Cold War atmosphere of Sputnik and the 'missile gap', Polaris was seen as a desperate 'need', and Raborn's skill was to ensure that no-one forgot that. But for Poseidon, with the USA nuclear arsenal already well exceeding the Soviet's in the mid 1960s, the 'need' was unclear (the potential ABM 'threat' never materialized), internally contested, and difficult to rally public or Congressional enthusiasm for. SPO no longer commanded priority.[118]

With SPO's dominance reduced, Poseidon became the first FBM to be seriously contested, not only within the 'establishment', but also in public where its MIRV development attracted much criticism. Also, while SPO was able to avoid a commitment to a hard-target Poseidon, and to retain the emphasis on assured destruction, the final outcome was a FBM force which fell a little short of providing the one (hard-target kill), but exceeded 'requirements' for the other (assured destruction). To many, especially those within the Navy connected with targeting, this increased the desire to make any future FBM have 'genuine' hard-target kill capability. Navy officers assigned to the Joint Strategic Target Planning Staff (JSTPS) soon tired of Air Force taunts about the Navy's 'firecrackers' - Poseidon's 40 kiloton warheads.[119]

But even as Poseidon began to enter service, counterforce advocates were to have another chance as the next FBM generation was planned. Preoccupied with rectifying Poseidon's difficulties, SPO would again have its authority challenged, but this time, on many key issues it would be the loser.

117. Sapolsky, 201.
118. Ibid, 224.
119. Interview.

Chapter 6

Strat-X, ULMS and Trident I

> **I considered that we'd better surrender to Rickover so that we wouldn't have to surrender to the Russians.**
>
> **Admiral Zumwalt.[1]**

While Poseidon development was still underway, and its final nature still not completely decided, consideration began of another generation of fleet ballistic missiles. Paradoxically, the path that leads to Trident II - the first Fleet Ballistic Missile in which hard-target kill capability would be a clear requirement - began with a study based on the criteria of a different era. That study was called Strat-X, and embodied the cost-effectiveness orientation of the 'systems analysis' of the McNamara era, and its emphasis on 'assured destruction'.

Strat-X and ULMS

The Strat-X study was a response by Robert McNamara's Deputy Director of Defense Research and Engineering, Lloyd Wilson, to Air Force pressures in the mid-1960s for a new, very large ICBM, provisionally called WS-120A; it may indeed have been initiated precisely to kill the Air Force missile.[2] Starting in late 1966, Strat-X was carried out by the Institute for Defense Analysis, and was submitted in August 1967. Its task was specified, in part, as:

> Strat-X is to be a technological study to characterize U.S. alternatives to counter the possible Soviet ABM deployment and the Soviet potential for reducing the U.S. assured-destruction-force effectiveness during the 1970's. It is desired that the U.S. alternatives be considered from a

1. Mary Schumacher, Trident: Setting the Requirements (Case Study C15-88-802.0, Harvard University John F. Kennedy School of Government, 1987), 10. I am very greatful to Mary Schumacher for supplying me with her work on Trident and with the Rhodes thesis (see next footnote).

2. F. Leary, 'ULMS: Strategic Emphasis Shifts Seaward', Space/Aeronautics (June 1970), 24-33, at 26; also Interview; Edward Rhodes, 'Trident: Bureaucratic Politics and Military Procurement' (unpublished thesis, Harvard College, March 1980), 12.

uniform cost-effectiveness base as well as from solution sensitivity to various Soviet alternative actions.[3]

Various strategic nuclear weapons systems were compared using criteria based on 'assured destruction'. How, it was asked, could the US most cost-effectively ensure the retaliatory 'delivery' to the Soviet Union of sufficient 'equivalent megatons'[4] to deter it - and do so on the assumption of possible Soviet developments such as ultra-accurate ICBMs and anti-ballistic missile defenses?

Both the Navy candidates - one submarine and the other surface-based - did well in this competition for the most cost-effective 'assured destruction' weapon system, and the Navy was instructed to continue studying them, but the Surface-Launched Missile System (SLMS) was dropped in 1968.[5] The submarine concept, known as ULMS or Undersea Long-range Missile System comprised a large, but not very fast, submarine carrying up to 24 missiles of 4500 to 6500 mile range held in canisters external to the pressure hull.[6] The use of canisters simplified hull construction and provided some launching advantages: 'Missiles may be released from the submarine at all speeds and depths up to the maximums, and missile firing may be delayed to avoid backtracking of trajectory so that submarine survivability is not inhibited by missile launch constraints.'[7]

An advanced development objective for ULMS was established by the Chief of Naval Operations on February 1, 1968, with Admiral Smith of SPO named as the Project Manager in March. Then in July, reflecting the interest in a possible surface-launched system, Admiral Smith's responsibility was increased to include all Navy strategic systems (not just

3. Volume I of Strat-X's 20 volumes is unclassified, The Strat-X Report (Arlington, Virginia: Institute for Defense Analysis, August 1967), 1.
4. For definition of equivalent megatonnage see Chapter 4, footnote 38.
5. Leary, 'ULMS', 26; Captain Dominic A. Paolucci (US Navy, Retd), 'The Development of Navy Strategic Offensive and Defensive Systems', United States Naval Institute Proceedings, Vol. 96 (May 1970), 223, also mentions the SLMS.
6. On Strat-X and ULMS, see J. Steinbruner and B. Carter, 'Organizational and Political Dimensions of the Strategic Posture: the Problems of Reform', Daedalus, Vol. 104 (Summer 1975), 131-54; N. Polmar and T. Allen, Rickover (New York: Simon and Schuster, 1982), chapter 26; also Strat-X, Vol I.
7. Strat-X, 84.

FBMs) and the Special Projects Office was renamed the Strategic Systems Project Office (SSPO).[8] During 1967-1969 SSPO carried out preliminary studies to define the ULMS technology based on the concept used in Strat-X.

In particular, Admiral Smith and SSPO sought to keep research and development costs low by utilising as much existing technology as possible. The long range of the missile would be obtained simply by increasing the volume of propellant used - the missile envisaged was to be about twice the volume of Poseidon - and the submarine size would grow accordingly, to about 18,000 tons displacement.[9]

The reactor envisaged to power the new submarine was an existing design, already tested in the attack submarine *Narwhal*. This again would help to reduce the cost and uncertainty involved in the development of new technology, especially when responsibility for that development would lie outside of SSPO's control, in the hands of Admiral Rickover, head of the Navy's Nuclear Propulsion Directorate. In addition the *Narwhal* reactor was a natural circulation design which at low speeds used convection rather than pumps to circulate the water-coolant. At the normal cruising speeds of an FBM patrol this was expected to make it significantly quieter than a conventional forced circulation design. This too accorded with Admiral Smith's desire to minimize the noise (and other observable characteristics) of the ULMS submarine so as to enhance its survivability against developments in Soviet anti-submarine warfare. However, because the *Narwahl* was only about a third the size of the proposed ULMS submarine, its 17,000 shaft horsepower (shp) reactor would only allow a top speed of about 18-19 knots, making it about four knots slower than the Polaris/Poseidon submarines.[10] But a high top speed was not considered an important attribute of an FBM submarine in Strat-X, nor by Admiral Smith.

Instead SSPO's ULMS design reflected the emphasis on cost-effectiveness and survivability through low observability. It did differ

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8. Paolucci, 223.
9. Schumacher, 5; Rhodes, 26.
10. Schumacher, 5.

from the Strat-X concept in one major respect, however, in that the missiles were to be carried internally in vertical launch tubes, as in the Polaris/Poseidon submarines. As usual Admiral Smith also sought to ensure that SSPO would be able to deliver what it promised on schedule. The choice of an existing reactor design not only greatly reduced the time and expense of a new research and development programme, but also reduced SSPO's reliance on others.

But, in marked contrast to Polaris, the development of the next generation FBM submarine was to be a divisive and contested battle, the outcome of which would affect the nature of FBM technology for many years. In this struggle issues of the 'technical' design of the new submarine interacted with the strategic role of the FBM and the relative power of the various organizations and individuals involved.

The Trident Submarine Debate

The submarines that carried Polaris and Poseidon had been uncontroversial. But ULMS reopened the issue that SPO had successfully closed with the choice of a modified existing attack submarine design to carry Polaris - the potentially deeply problematic relationship with Admiral Rickover, 'father' of nuclear propulsion.

Rickover became aware of the ULMS plans early in 1970 when SSPO asked his nuclear propulsion directorate for information on the weight and size of the *Narwhal* reactor. He immediately objected, arguing that the ULMS submarine should be able to reach a top speed of at least 24 knots. Above this speed active sonar had failed to operate effectively in tests, and so even though Soviet attack submarines would be faster (and so able to outrun a 24-knot submarine), they would be 'blinded' in chasing at such speeds.[11]

To meet the 24 knot 'requirement' Rickover instead proposed powering each submarine with two 30,000 shp reactors of a design that was still to be tested. Although many remained sceptical of the argument for speed (the 'blinding' effect was based on limited data) Admiral Smith was

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[11]. Rhodes, 28.

put under considerable pressure by the Office of the Chief of Naval Operations to reach an agreement that would allow the submarine design to be settled. With SPO's technical authority already being questioned for the first time in the Poseidon programme, and with its autonomy much reduced from the days of Polaris, Admiral Smith chose not to fight a divisive battle with Rickover which might further delay and endanger the ULMS programme.

Despite his personal misgivings Admiral Smith was forced to concede that his area of expertise was the missile system, whereas that of Rickover 'was basically shipbuilding in its various aspects, particularly propulsion'.[12] This logic compelled Admiral Smith to accept that once he had set the missile characteristics, and most importantly their weight, then Rickover should design the submarine. This agreement led, by March 1970, to a 'compromise' which very much favoured Rickover. The ULMS submarine was to use his favoured twin natural circulation reactors to give the submarine a top speed of 26-27 knots. To accommodate these large reactors, as well as the large 6000-mile missiles (in launch tubes nearly three and a half times the volume of Poseidons) required a huge submarine with a 50-foot hull diameter and displacing about 30,000 tons.[13]

But this was far from the end of the matter. When the Deputy Secretary of Defense David Packard learnt about the proposed submarine in September 1970 he 'rejected the idea of such a large submarine emphatically and categorically'.[14] It became clear that approval of ULMS would require some compromise towards Packard's preferences, which were made clear to the Navy: 'Minimum detectability as well as minimum cost will be given top priority in OSD reviews'.[15]

ULMS now became a more pressing concern for the newly appointed Chief of Naval Operations, Admiral Elmo Zumwalt. In October 1970 he revitalized the ULMS Steering Group which worked out a smaller,

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12. Smith quoted in Schumacher, 8
13. Schumacher; also Rhodes, 31-32..
14. Rhodes, 33.
15. Conversation with David Packard as recalled by R. W. Cousins (Vice Chief of Naval Operations), 'Memorandum for the Chief of Naval Material. Subj: ULMS Studies. Ser 00308P31' (November 10, 1970), Cited in Rhodes, 128, note 11.

less expensive ULMS submarine design, basically by scaling down the Smith/Rickover compromise. Recognising the necessity of maintaining Rickover's support the new design retained his favoured 30,000 shp reactor, but used only one rather than two. Although it still had a displacement over one and half times that of the latest Polaris/Poseidon submarines, the 640-class, it was dubbed the 'Super-640', apparently in an attempt to minimize the difference. It also differed significantly from the Strat-X concept in that the missile tubes were only about ten percent larger in volume than Poseidon's.[16]

Zumwalt himself favoured repeating the first Polaris submarine construction method by simply adapting the latest attack submarine design, the 688-class, and installing a missile section into it. Since the 688 design was already developed (including the reactor) this option eliminated much of the research and development costs, and seemed to offer considerably cheaper submarines, which was Zumwalt's main concern. However, Zumwalt decided that Rickover's support was the critical factor in determining the feasibility of the ULMS submarine. What would be the optimum design in 'technical' and strategic terms was something over which there was considerable disagreement, but one thing was for certain - Rickover was a hard reality. Zumwalt needed to enroll his support for ULMS:

> it was clear to me that Rickover would never support anything that didn't have his huge ... new reactor in it. It was clear to me that he would have been willing to take any delay to get it.[17]

The Super-640 satisfied Rickover, but had the unintended consequence of undermining the rationale for developing *any* new submarine. When faced with the prospect of a submarine which could only accommodate launch tubes not much bigger than Poseidon's, SSPO set about investigating whether they could meet Strat-X's range/payload goals with a smaller missile than previously envisaged. By abandoning the commit ment to minimize research and development it looked possible to develop a new missile which could provide longer range.

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16. Schumacher, 10.
17. In Schumacher, 10.

Moreover, it might be possible to develop this so that it would not only fit the Super-640 tubes, but also those of the existing submarines, currently carrrying Poseidon. Known as EXPO (extended range Poseidon), this new proposal offered the prospect of deferring submarine construction altogether. Retrofitting EXPO to existing submarines would increase their searoom and thus survivability against any short-term developments in Soviet anti-submarine warfare capabilities. EXPO gained the support of Admiral Smith because it obviated the need for the Navy to rush into development of new submarines based on rushed studies and a possibly premature assessment of Soviet developments. Rather than the Navy pushing the development of the ULMS submarine so vigorously, Smith felt that the impetus for such a major programme should come from above. If more submarines were needed in the meantime then why not simply build a few more of the 640-class and equip them with either Poseidon or EXPO missiles.[18]

But not surprisingly EXPO found little favour with Rickover or Zumwalt. Zumwalt's concern over what he called 'the tremendous growth of the Soviet threat' was the reason why he was prepared to give in to Rickover over the Super-640. Zumwalt wanted to start construction of a new submarine as soon as possible. To him the EXPO option was 'just peeing around', as he put it at a meeting of the ULMS Steering Group on January 27, 1971.[19] Viewing EXPO as 'a way of defeating construction of a new submarine' he made it quite clear at that meeting that the Super-640 decision was final - OSD was not even to be informed of the EXPO option.[20]

In March 1971, apparently in response to a Rickover proposal, Zumwalt set up a new office to manage the ULMS programme and appointed Rear Admiral Harvey E. Lyon - considered by many to be a Rickover protegé - as ULMS project manager, or PM-2. Zumwalt felt this necessary to coordinate ULMS development because 'it was evident that Levering Smith and Rickover could not talk to each other in a reasonable way'.[21] But in effect it meant that Admiral Smith, as PM-1, was

18. Rhodes, 41.
19. Rhodes, 44.
20. Rhodes, 44.
21. Schumacher, 12.

subordinated to the role of missile developer. From the highpoint of SPO's autonomy during the development of Polaris, the office, now SSPO, had lost much of its control over the development of ULMS.

Following the establishment of PM-2, the Electric Boat Division of General Dynamics Corporation was awarded an initial contract for submarine design and the ULMS Ship Acquisition Project was set up to oversee it. ULMS characteristics were worked out for submission to OSD with initial operational capability (IOC) to be in 1979 or 1980. Particularly significant was growth in the size of the missile - by about six inches in diameter and four to five feet in length - without any change in mission requirements.[22] Unlike the earlier design, which was not that much larger than EXPO, the new dimensions meant a larger submarine would certainly be required. Similarly, the reactor size was increased from 30,000 to 35,000 shp, perhaps to help avert any suggestions that an existing 30,000 design (forced, not natural circulation) already deployed in attack submarines be used to save research and development costs.[23]

But still Deputy Defense Secretary Packard remained sceptical. On learning of the EXPO proposal - apparently via a civilian staffer in SSPO who felt no particular loyalty to or fear of Zumwalt's injunction to suppress it[24] - he began to favour an approach very similar to Admiral Smith's. He would commit to ULMS development, but on a delayed time-scale with IOC no sooner than 1984, and in the meantime suggested that EXPO development - with an IOC of 1978 - would provide interim cover to ensure FBM submarine survivability.[25]

Packard's proposal formed the basis of one of the options presented in a twenty-page Development Concept Paper (DCP No. 67) that was prepared by OSD and the Navy and released on September 7, 1971. It presented five options 'for maintaining the deterrent effectiveness of our sea based forces':[26]

22. Steinbruner and Carter, 138.
23. Ibid.
24. Rhodes, 60.
25. Rhodes, 62.
26. Schumacher, 13; also see SASC FY 1973, Part 5 R&D, (Washington, DC: US GPO, 1972), 2634-36.

1. Do nothing (cancel ULMS and extended range Poseidon).

2. Extended range Poseidon, IOC about CY [calendar year] 1977.

3. ULMS, with IOC about CY a) 1979, b) 1980, c) 1981, and d) 1982.

4. ULMS, IOC about 1981, but with a parallel development of an extended range Poseidon missile to permit the option of deploying extended range Poseidon in about CY 1977.

5. Extended range Poseidon missile, with IOC about CY 1977, followed by ULMS with a delayed IOC (about CY 1983).

To all concerned options 4 and 5 were considered the only real alternatives.[27] Option 4 would give the Navy (not including SSPO, of course) what they wanted - a firm commitment to ULMS with only token reference to EXPO development, but not necessarily deployment. Option 5 embodied the preferences of Packard and Admiral Smith. Attached to the DCP were recommendations from other interested parties, which ranged from that of the Joint Chiefs of Staff (who favoured the Option 3, ULMS only approach which gave 'the Navy' what it wanted at the least cost to the Defense budget) to that of the Assistant Secretary of Defense for Systems Analysis (who favoured immediate engineering development of EXPO, but wished to keep the whole question of ULMS design, and indeed of whether to choose ULMS at all, under review for several more years).[28]

A week later on September 14, OSD released a 'Secretary of Defense Decision' on ULMS, said to have been drafted by Packard.[29] This was presented as approving a modified option 4 from the DCP, but in substance was nearer to Packard's original choice, option 5. The term EXPO was diplomatically dropped, but emphasis was given to the development of a missile with 'a range as near to 4,000 miles as possible while being compatible with the present configuration of Poseidon boats'.[30] This

27. Rhodes, 65.
28. Rhodes, 66-67.
29. Rhodes, 68.
30. Quoted in Rhodes, 68.

missile, referred to as ULMS I, was to have an IOC of 1977, whereas ULMS II would be a longer-range 'optimized missile design for deployment in a new submarine'.[31] No exact IOC was set for either ULMS II or the new submarine, whose characteristics still remained to be defined:

> The parameters of the new boat which are affected by the missile characteristics should not be established until work on the missile program has established range, performance and size parameters for the new missile. Development of subsystem improvements, propulsion, quieting, etc., can and should proceed in parallel with the new missile development. The objective of the ULMS program should be to bring in a new force of reasonable cost in the early 1980s.[32]

Whereas quietness was to be given 'first priority' in the submarine design, no mention was made of speed or power requirements.[33] Although EXPO was repackaged as ULMS I, the decision looked to be a blow to Rickover and Zumwalt and a satisfactory outcome for Admiral Smith and SSPO.

But this decision did not last long as international (and domestic) politics now came to play a role. In October 1971 Packard and Secretary of Defense Melvin Laird were persuaded by President Nixon that US FBM force development should be accelerated to allow him some leverage in the SALT negotiations with the Soviet Union.[34] As well as providing a 'bargaining chip' to use against the Soviet Union in SALT (and possibly later negotations), an initiative in FBM submarine construction was also considered useful to mollify possible 'hawkish' right-wing and service opposition to SALT ratification.[35]

The quickest way to begin increasing US FBM forces was, of course, either to build more 640-class submarines or to convert attack submarines

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31. SASC FY 1973, 2636.
32. Quoted in Schumacher, 15.
33. Schumacher.
34. This was officially confirmed on December 2, 1971 when National Security Advisor Henry Kissinger 'requested Laird to give favourable consideration to "an expanded strategic submarine program" in a way that was highly visible to the Soviets'. H. Kissinger, The White House Years (Boston: Little, Brown & Co., 1979), 1129. Apparently Paul Nitze had relayed Soviet concern over US FBM developments from the SALT negotiations during late 1970 and early 1971. See Elmo R. Zumwalt, Jr. On Watch (New York: Quadrangle, 1976), 154.
35. Steinbruner and Carter, 140.

already under construction, but these options were known to be unacceptible to key Navy factions, and especially to Admiral Rickover. Attempting to impose them on a recalcitrant Navy would be unlikely to mollify anybody. Instead Packard agreed to reshape his ULMS package.

He first consulted with Rickover, who on October 31 replied affirmatively to Packard's inquiry about the feasibility of accelerating the ULMS submarine construction schedule. According to Rickover, the lead ship could be ready by late 1977, and beginning in 1978 construction could proceed at the rate of three a year.[36] Packard then instigated a Navy study to compare the options for accelerating FBM submarine construction, and to provide justification for the one he had already chosen, ULMS acceleration.[37] As recalled by an SSPO source: 'The study was a sham. Packard had already made up his mind. We were simply going through the motions. We all knew what the answer was.'[38]

But OSD was not simply imposing the President's wishes on the Navy. The FBM acceleration issue also provided an opportunity to strike a blow against the interference of Kissinger's National Security Council (NSC) in military matters. Kissinger had set up the Defense Program Review Committee (DPRC) in the National Security Council in an attempt to gain some control over the Department of Defense.[39] Naturally Secretary of Defense Laird sought to minimize the role of the DPRC, and in this case National Security Council staff were unable to obtain details of the studies that were circulating in the Pentagon.

Thus the favoured approach at the White House - which was apparently to build more Polaris/Poseidon type submarines - was simply ignored by OSD. On December 26, without consultation with the President, Laird ordered ULMS acceleration.[40] This was leaked to the press on January 12, 1972 presenting the President with a *fait accompli*, which

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36. Rhodes, 80.
37. A November 1, 1971 memorandum from the Deputy Sec retary of Defense to the Secretary of the Navy directed the Navy to: 'Initiate a study immediately of the alternative ways of providing early increases in the deployment level of our sea-based strategic offensive weapons'. See SASC FY 1973, 3188.
38. Schumacher, 17.
39. Kissinger, 394-97.
40. Rhodes, 85.

could not realistically be challenged. To do so would not only involve publicly contradicting the Secretary of Defense, but also taking on the powerful factions of the Navy, including Zumwalt and Rickover. The Navy backed up their claim that ULMS submarines could be ready virtually as soon as extra Polaris/Poseidon submarines by drastically shortening the ULMS development time, almost literally overnight. A staff member of the Trident Ship Acquisition Program Office recalled what happened over a weekend in early January 1972:

> The delivery date on that Monday was December 1977; on the preceeding Friday it had been December 1981. In that one fell swoop, they [the Office of the Chief of Naval Operations and the Navy Secretariat] had taken . . . the program parameters and what have you, and for political reasons, which were obviously SALT, had [changed] them.[41]

Aware that Packard was determined to back an accelerated ULMS programme, the ULMS Project Office was able to set the design to suit its priorities. This was announced by Admiral Lyon on November 9, 1971, and included a missile tube sufficiently larger than the Super-640 version to make it completely unable to fit an existing or modified Polaris/Poseidon submarine. The ULMS submarine was to have a 42-foot hull diameter, would displace 18,700 tons (compared to the Super-640's 14,000), and have a reactor of 35,000 shp (compared to 30,000 in the Super-640).[42] On May 15, 1972 ULMS was renamed Trident.

The number of missiles to be carried by each submarine was twenty-four or half as many again as in the original FBM submarines. Navy parametric studies supported the obvious conclusion that the more missiles per submarine, the cheaper the deployment cost per missile. But, according to Admiral George Miller (head of Navy strategic planning since the 'Great Circle Group' was set up and an advisor to Strat-X), the decision to go for twenty-four was 'arbitrarily made, just to make the expensive sub look more cost-effective'.[43] However, although more missiles per

41. Quoted in M. Schumacher, 'Trident Contracting (A): Drafting the Request for Proposals' (Kennedy School of Government Case Program, Draft, 1988), 12.
42. Schumacher, 'Trident Contracting (A)', 18.
43. Quoted in M. Mintz, 'Depth Charge: Cost Overruns on New Trident Sub Leave a Muddied Wake', Washington Post (October 4, 1981), reprinted in Dina Rasor (ed.), More Bucks, Less

submarine was more cost-effective, it also, to some, raised the worry that the Navy was putting all its eggs in too few baskets, should Soviet anti-submarine warfare improve. For example, Admiral Smith of SSPO 'would have favoured a smaller submarine than the Trident - wouldn't have put as many missiles in it'.[44]

It was this fiercely contested result of bureaucratic intrigue that was presented to Congress for funding. There too the FBM programme no longer commanded unquestioning support. Poseidon had already raised some doubts, and although the general principle of eventually building new FBM submarines was not in question, many opposed the Administration's acceleration plans as being too hasty. Ironically, the Administration now found it necessary to put pressure on some of the 'hawks' whose expected reaction to SALT it had originally intended to placate by ULMS acceleration. In both 1972 and 1973 tied Senate votes on amendments to cut Trident funding were only overturned after intensive lobbying of key conservatives - John Stennis in 1972 and Barry Goldwater in 1973 - by the Administration and the Navy.[45]

Despite the tenuous consensus in support of Trident, funding was approved. However, some of the recommendations of the Research and Development Subcommittee of the Senate Armed Services Committee were taken up by the new Secretary of Defense, James Schlesinger:

> The Secretary of Defense has restructured the program, consistent with the actions of Congress, but has gone even further. He has adopted the recommendations made by the Research and Development Subcommittee last year [1973] to slow the pace of submarine construction from three to two per year, and has approved the backfit of Poseidon submarines with the C-4 missile, beginning in fiscal year 1979, now planned for ten submarines. Previously, this was approved only as an option for initiation in the early 1980s.[46]

Bang: how the Pentagon Buys Ineffective Weapons (Washington, DC: Fund for Constitutional Government, 1983), 211-23, at 214.

44. Interview.

45. See J. W. Canan, The Superwarriors: The Fantastic World of Pentagon Superweapons (New York: Weybright and Talley, 1975), 184-86; Zumwalt, 158-63.

46. Quoted in Canan, 187.

This decision to make a firm commitment to backfit the Trident I missile into existing FBM submarines was very much inspired by the wishes of Admiral Smith of SSPO, who did not want the deployment of that missile to be limited by any delays in the Trident submarine construction which lay beyond his control.[47] It turned out to be a very prescient decision.

Building the Trident Submarine

Through the late 1970s the Trident submarine programme was dogged by delay, cost overruns and litigation. While it was originally hoped that the first Trident submarine, the USS *Ohio*, would be delivered in December 1977, it in fact began sea trials only in June 1981.[48] Compared to the success story of the original Polaris, the Trident submarine construction was a public relations disaster.

Many of the problems stemmed from the decision to accelerate Trident construction, that Rickover had assured Packard about in late 1971 and that had led the delivery date to be advanced four years over one weekend. Although widely disliked within the Navy, Admiral Rickover was also respected for his technical competence. He not only assured Congressional doubters that the first Trident submarine could be delivered within the new schedule - in time for the expiry of the SALT Interim Agreement in 1977 - but also committed the Navy to unusual costing arrangements.

First-of-a-kind lead ships, like the first Trident submarine, the USS *Ohio*, would normally be built under some kind of cost-plus contract. With so many uncertainties in the design of a lead ship it was generally considered unreasonable to insist that a fixed price be met. Typically a cost-plus contract would provide for all relevant costs to be reimbursed with the addition of either a fixed fee or an incentive fee dependent on how nearly a target cost was met.[49] Admiral Rickover, however, was a firm believer in fixed-price contracts which committed the Government to pay the fixed price agreed on (usually providing greater profit margin to the

47. Interview.
48. Polmar and Allen, 579 and 576.
49. See Schumacher, 'Trident Contracting (A)'; Mintz, 215-17.

contractor than cost-plus arrangements), but in which the contractor bore at least some of the financial risk involved.[50] He argued that the Trident submarines would be similar in nature to those built for Polaris, and so construction would be a fairly straightforward task, involving little risk.

The Trident acceleration was thus 'sold' to Congress in 1972 on the understanding that delivery by 1977 was a simple matter, which could be achieved as cheaply as possible by a competitively-tendered fixed-price contract. But this view was not held by the only shipyards capable of building the submarine - General Dynamic's Electric Boat Division at Groton, Connecticut and Newport News in Virginia. The Navy received their bids on November 5, 1973 and neither complied with the terms outlined in the Navy's RFP (request for proposals). Newport News offered delivery in May 1981 under a cost plus fixed fee contract. Electric Boat were prepared to attempt delivery for April 1979 and wanted a cost plus incentive fee contract.[51] Neither, of course, were acceptable to the Navy and Rickover, who had been promising delivery by December 1977 under a fixed-price contract.

In fact both shipyards already had problems building the Navy's latest nuclear-powered attack submarines, the 688-class. Newport News was having great difficulty with the first five 688 boats, and when a second flight of eleven submarines was tendered in 1973 their bid was considered too high by the Navy.[52] Newport News had other work, including merchant shipbuilding, and so did not need the financial risk of bidding low. Electric Boat, however, had only one product, submarines, and only one customer, the Navy. They too had problems with the 688. The ambitious new chairman of General Dynamics, David Lewis -after considerable pressure from Rickover - had already 'undercut' Newport News on the bid to build the second eleven 688s.[53] This was in October

50. Typically fixed-price contracts might have a ceiling price (say, about 30% more than the agreed cost) up to which the Goevernment and the contractor would share the extra costs.
51. M. Schumacher, 'Trident Contracting (B): Evaluating the Bids', (Kennedy School of Government Case Program, Draft, 1988), 7.
52. Ibid, 5-6.
53. Patrick Tyler, <u>Running Critical: The Silent War, Rickover,and General Dynamics</u> (New York: Harper & Row, 1986), 131-33.

1973, just before the Trident bids were due, and the shipyard was already committed to build many submarines at a price and schedule that it would not be able to meet. Then in December, following their unacceptable cost-plus Trident bid of November, Rickover persuaded Lewis to resubmit their bid as a fixed-price contract. The risk of construction uncertainties causing a large overrun would, Rickover reassured Lewis, be catered for by simply making the fixed-price sufficiently high.[54]

After prolonged negotiations between the Navy and Electric Boat it was agreed to settle on a fixed price contract with a target price for the first submarine of $285,400,000 but with ceiling price (up to which the Navy would still pay at least 85% of the overrun) of over half as much again, $384,000,000.[55] Electric Boat also promised to make its 'best efforts' to deliver by December 1977 and guaranteed delivery by April 1979, but in neither instance was there to be any penalty for being late.[56] Certainly, the contractor, Electric Boat Division of General Dynamics was less than candid about its ability to meet the demanding specifications of the new submarines on schedule.[57] The contract, which offered the prospect of more work than Electric Boat could (literally) cope with, was signed on July 25, 1974.

To save its, and Rickover's, face the Navy had played on the ambitions of Electric Boat's management to push them into a cosmetic contract which seemed to meet the fixed-price, competitively-tendered, December 1977 delivery-date promised to Congress. In reality it met none of these, and despite repeated attempts to suppress Electric Boat's difficulties, the non-delivery of the *Ohio* on the promised schedule inevitably brought the Trident programme into disrepute. In the late 1970s Electric Boat found itself committed to building too many submarines, too quickly. Expectations of increased productivity, which had encouraged lower bids, simply could not be realised. The *Ohio* was eventually delivered in October 1981 after the construction schedule had been officially extended six times.

54. Tyler, 134-35.
55. M. Schumacher, 'Trident Contracting (C): Negotiating the Contract', (Kennedy School of Government Case Program, Draft, 1988), 8.
56. Mintz, 218.
57. See Tyler.

Trident I C4

Admiral Smith's worst fears had come true and had justified his concern over making the Trident I missile small enough to be back-fitted into the majority of the existing FBM submarines. Although largely excluded from the Trident submarine programme, SSPO retained jurisdiction over maintaining the existing FBM fleet and over missile development. In line with SSPO's preferences Trident I was to be a realization of the EXPO concept, providing extra range within the size constraint presented by the exisiting FBM submarines.

In March 1971 the Navy still apparently viewed the FBM as an essentially urban-industrial weapon, referring to 'the assured destruction role that we are building ULMS for at this time.'[58] Assured destruction was the criterion on which Strat-X had been based and it was the role which was considered paramount at SSPO. Others, however, including some in key positions favoured increasing the counterforce capability of the sea-based portion of US ballistic missiles.

One of these was John Brett, Under-Secretary for Strategic Systems, who was responsible for transforming Packard's vague direction into a more detailed specification for the C4. Brett could not, of course, impose a specification unilaterally. But he was well-placed to intervene - for he was a former Kearfott engineer, a protagonist of Kearfott's stellar-inertial system, who felt that the US badly needed a significant counterforce capability. He also appreciated - following the cancellation of the stellar-inertial accuracy enhancement of Poseidon - that Congressional opinion needed to be taken seriously.

The strength of the anti-counterforce lobby in Congress meant that it would be unwise to push the C4 as a hard-target killer. The specifications of C4 could, however, be set in such a way that enhanced navigation and guidance capability was still required, but on 'assured destruction' grounds rather than 'counterforce' grounds. A longer range missile - which provided more sea-room for the submarine to patrol in

58. SASC FY 1972, Part 3, R&D (Washington, DC: US GPO, 1971), 2233.

and so would alleviate concerns about Soviet anti-submarine warfare developments - required improved navigation and guidance capability simply to prevent deterioration in accuracy. Similarly the ASW 'threat' could be taken as necessitating the capability to operate for much longer periods of time, up to several weeks, without resort to navigational resets - again necessitating enhanced navigation and guidance. The navigation and guidance specifications for the C4 were thus set as follows: system accuracy of C4 at 4000 nautical miles should be as good as Poseidon at 2000 nautical miles, and submarine navigation should be able to operate for periods of thirty days without external reset.[59] In Brett's opinion, this left no option but that Trident I would require stellar-inertial guidance.[60] It also meant that under the best possible circumstances, at shorter ranges and soon after a navigation reset, C4 accuracy would be considerably better than Poseidon. Up to this point SSPO, whose main concern was longer range, was considering simply modifying Poseidon's Mk3 guidance system, even at some loss of accuracy.[61]

Warhead size, however, raised the hard-target issue in a way that was harder to 'fudge'. As with Poseidon, the supporters of hard-target capability in the Offices of the Secretary of Defense and Chief of Naval Operations wanted a larger-yield warhead than Poseidon Mk3's 'small' 40 kilotons. They were, reportedly, joined by Navy officers from the Joint Strategic Target Planning Staff, who were tired of Air Force jibes about the Navy's 'firecrackers'.[62] As before, SSPO was unconvinced, seeing little reason not to use basically the same reentry vehicle and warhead as on Poseidon.[63] This time round SSPO had a powerful 'technical' argument to mobilize against a very large warhead: the missile design required to get the longer range from the same size. A third-stage rocket motor was added for the first time, and instead of it being below the post-boost vehicle containing the guidance system and warheads, as was conventional, the

59. Interview.
60. Interview.
61. Interview.
62. Interview.
63. In March 1972 testimony to the Ad Hoc Research and Development Subcommittee of the Senate Armed Services Committee, Admiral Levering Smith noted that: 'The reentry body for the ULMS I missile is based on extension of the Poseidon Mk 3 design. The Mk 3 was limited to a maximum range of [deleted] miles and therefore increased heat protection and structural strengthening will be required for the reentry body to survive the reentry environment at ULMS I ranges.' SASC FY1973, Part 5 (USGPO, 1972), 3157.

third-stage motor went effectively to the top of the missile, with reentry vehicles and guidance system, etc., arranged round it. Reentry vehicle size was limited by the size of this annular ring, and this in turn limited the maximum yield warhead possible with the current state of technology. SSPO's view was that there was little to be gained from moving to a slightly larger warhead, especially given the expense. There was also some argument over whether the Poseidon Mk3 reentry vehicle was suitable for the longer ranges. Its heatsink design meant that on some trajectories at the longer range the heating would be excessive, but it was still felt that adequate trajectories would be available. In the end SSPO went for a new reentry vehicle and somewhat larger warhead, reportedly because 'they recognized the political benefit of agreeing with OSD'.[64] Compared to the Poseidon's typical loading of ten 40 kiloton warheads, Trident I has a maximum loading of eight 100 kiloton warheads.[65]

With the decision to build a new warhead/re-entry vehicle combination, OSD's hard target advocates now were able to get not only the (marginally) higher yield, but also a re-entry vehicle with a higher ballistic co-efficient and hence less accuracy loss due to dispersion.[66] To survive at the ranges desired and also have a high ballistic coefficient necessitated the choice of an ablative design for the Mk4 re-entry vehicle. Various alternative designs were tested during 1974 and 1975 using surplus Atlas and Minuteman missiles. The final choice has 'a tape-wrapped carbon phenolic (TWCP) heatshield bonded to a thin-wall aluminium substrate for the shell and a graphite nosetip'.[67] With nuclear hardness less important than in Poseidon it was possible to use a plug type nose tip which is inherently stronger than the shell type used in the Mk3.[68]

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64. Interview.

65. T. B. Cochran, W. M. Arkin and M. M. Hoenig, Nuclear Weapons Databook, Vol. 1: US Nuclear Forces and Capabilities (Cambridge, Mass.: Ballinger, 1983), 142.

66. The ballistic coefficient - the weight-to-drag ratio - of the Trident Mk4 is said to be 1800 lbs/sq. ft., almost as high as the Minuteman III Mk12A. M. Bunn, Technology of Ballistic Missile Reentry Vehicles (Cambridge: MIT Program in Science and Technology for International Security Report No. 11, March 1984), 6-7.

67. L. Smith, R. H. Wertheim, R. A. Duffy, 'Innovative Engineering in the Trident Missile Development', The Bridge (National Academy of Engineering, Vol. 10, No. 2 (Summer 1980), 10-19, at 19.

68. Interview.

Whereas Poseidon's modest range goal had required only a relatively conservative approach to propulsion, the perceived need to provide more sea-room to counter developments in Soviet ASW pushed the Trident I missile design. Almost twice the range was desired from a missile of about the same size and weight. The four general ways possible to do this were followed: decreasing inert weight in the missile; increasing the volume available for propulsive energy; increasing the usable energy per unit volume; and increasing the delivered impulse per unit usable energy.[69]

This approach led to the development of lighter components throughout the missile, including the guidance system, electronics, the post-boost vehicle or 'bus' and the chamber cases. Weight reductions in the 'bus' stage provide the greatest range increment and led to the choice of graphite-epoxy composite material, which in 1973 became available in a suitable form.[70] The bus is designed to carry the reentry vehicles and deploy them onto the trajectories required to bring them to the designated targets. It contains the equipment section, comprising the guidance and flight control systems, and the post-boost propulsion system which is required to vary the attitude and velocity of the 'bus' as it dispenses the reentry vehicles. In designing the 'bus' some accuracy was traded off against reduced weight:

> In designing for range, the 'bus' structure was designed to be of minimum weight for structural integrity with adequate margin. The optimized graphite cone structure, as an outcome, had vibrational modes which added a statistically bounded, but not exactly predictable on a body-by-body basis, increment to deployment velocity. This increment of course translates to an addition to the CEP...While neither large nor affecting performance relative to the goal, this deployment inaccuracy was nevertheless identifiable and could have been traded for less range.[71]

Because of the critical effects of weight savings in the 'bus' the type of propellant it used was the object of some debate. Lockheed suggested that the savings in weight (though not volume) provided by moving to

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69. Smith et al, 12.
70. Ibid, 14.
71. Letter from Captain Steven Cohen to Donald MacKenzie, 2 December 1986.

liquid propellant, as used in Air Force designs, outweighed any safety concerns. SSPO disagreed, placing the highest priority on system safety, even at some loss of range. Instead a solid propellant system was retained, though one which allowed a means of 'throttling'. This was 'a solid propellant gas generator which burns slower at lower pressure when less thrust is needed as when changing attitude, when no change of velocity vector is needed, or when making vernier changes, but burns rapidly at a higher pressure when high thrust is needed to change the vehicle velocity vector.'[72]

The bus structure takes the form of a squat cone through which a third rocket stage protrudes. The addition of this third stage, small though it is, provides a greater addition to range than simply increasing the propellant carried in the second stage by the same amount. However, situating the third stage through the post-boost vehicle made thrust termination and separation more problematical. Venting the third stage to provide thrust termination would have been difficult without inflicting high shock levels on the equipment section, flying it out the front would expose the vehicle to high heat and force levels, and backing it out looked to be difficult to test when the third stage could have differing amounts of fuel left. The solution devised was elegant. With what was called a General Energy Management System (GEMS), the guidance computer would shape the missile trajectory to use up all the propellant in the third stage. This obviated the need for a thrust termination system, eliminating that potential source of reliability and accuracy reductions, and left a constant weight third stage motor case. Testability was then designed in: 'By sizing the thrust ejecting the empty third stage from the post vehicle to accelerate at one g, this new feature could be easily ground tested rather than depending primarily on flight testing'.[73]

Another consequence of the third stage positioning, along with the desire to utilize the launch tube volume to the full, was an unusually blunt nosed missile. The extra aerodynamic drag experienced during the boost phase would have reduced the range achieved and this concern led to the development of an aerospike to reduce drag. Self-contained, to

72. Smith et al, 15.
73. Ibid, 17.

avoid interface problems, the aerospike extension is powered by a small solid propellant gas generator triggered by the acceleration sensed as the missile is ejected from the submarine.[74] The optimum length for the aerospike was derived from experimental data; indeed, in 1984 there was 'still no theoretical means of predicting spike effects'.[75] Constructed of laminated Sitka spruce the aerospike is said to add 300 nautical miles to the missile's range.[76]

Meeting the range goal also required major technical advances in the first and second stage motors, to increase propulsive impulse whilst reducing inert weight. Again the joint venture of Hercules and Thiokol won the competitive tender against rival bids from Aerojet, for the first two stages, and United Technology Centers, for the third.[77] The propellant cho sen for all three stages was a development of the composite double base type which permitted a higher level of solids (the fuel and oxidizer), thus giving both greater density and specific impulse.[78] SSPO were pushing to obtain the greatest range possible, and had an internal range goal which exceeded the official 4000 nautical miles.[79] In doing so they pushed the motor designs too far.

Particularly alarming was an unexpected and unprecedented second stage motor detonation during a static test firing in May 1974. For over a year this became the focus of the development programme, as 'extensive analysis, laboratory experimentation, and large-scale motor tests were conducted to gain an understanding of the mechanism involved'.[80] The apparent cause was failure of the motor casing at the high pressures involved, leading to shear of the propellant away from the chamber wall and break up of the propellant. This rapid formation of a large propellant surface area in a confined space then initiated the detonation.[81]

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74. Ibid.

75. J. P. Reding and D. M. Jecmen, 'An Advanced Aerospike to Minimize Nose Drag', Lockheed Horizons, 15 (1984), 46-54, at 47.

76. 'Trident Subsystem Tests in Final Phase', Aviation Week & Space Technology Vol. 103 (November 3, 1975), 34-38, at 37.

77. 'Updated Propulsion System Seen Extending Trident Range', Aviation Week & Space Technology Vol. 98 (June 18, 1973), 19.

78. Smith et al, 18.

79. Interview

80. Smith et al, 18.

81. Ibid.

Even with a less than complete understanding of the mechanism, SSPO set in train corrective measures aimed at generally improving the uniformity and quality of components, and changing the propellant formulation to make it less energetic and less friable. In so doing they gave up some range to ensure the safety of the system. As Admiral Levering Smith testified to Congress in 1976: 'the solution to the detonation mechanism that we have identified resulted in our adopting a somewhat less energetic propellant with some loss of range'.[82]

But solution of the detonation problem did not end the propulsion difficulties that were to contibute to the poor public image that the Trident programme also acquired from the submarine construction delays. Following deployment of the missile in 1979 there were a number of first stage motor failures during test flights which led Undersecretary of Defense for Research and Engineering, Richard DeLauer, to describe their performance as 'lousy'.[83] A defect was identified as causing the failures and in 1984 a programme to recall 'suspect' motors was begun, and changes were made in propellant processing and in first stage insulator thickness. These changes were considered to provide a greater performance margin against such defects.[84]

Despite these propulsion difficulties Trident I achieved its range goal. However, they did make inert weight reductions throughout the system more critical, as 'the performance loss associated with the use of less energetic propellants than originally intended increased the need for greater performance contributions by all other areas.'[85] A newly developed material, Kevlar, was chosen for the chamber cases of the three rocket motors because of its high strength-to-weight and modulus-to-weight ratios. Weight reductions were stressed throughout the missile, including in the guidance system.

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82. SASC, FY1977, Part 12, R&D, (Washington, DC: US GPO, 1976), 6617.
83. Quoted in 'Trident failures "not hitting readiness"', Jane's Defence Weekly (September 1, 1984), 308.
84. See '73 Trident I's Withheld from Fleet due to Defects', Defense Daily, Vol. 135, No. 32 (August 16, 1984), 249-50.
85. Smith et al, 18.

Weight-saving also shaped the design of the missile electronics, as did compactness, low-power operation, radiation-hardness and reliability. For example, the relative compactness of electronic components went from a density of 16 equivalent parts/cubic inch in Poseidon to 480 parts/cubic inch in Trident I (Polaris A3 had 4 parts/cubic inch).[86] The combination of characteristics desired by Lockheed proved to be harder to manufacture than expected, and some of the intended components were unavailable for the early test missiles.[87] These difficulties with electronics and propulsion led to delays which caused the Trident I IOC (initial operating capability) date to be adjusted twice, first by six months, and then by five.

The Mk5 Guidance System for Trident I C4

As intended by John Brett, the Mk5 guidance system developed for Trident incorporated a star sensor mounted on the stable platform of the inertial measurement unit, together with the gyroscopes and accelerometers. This stable member was held in a four gimbal system rather than with three as in Poseidon and Polaris. This allowed one gimbal to be used for optical alignment with the SINS whilst another could be devoted to elevation of the star sensor through the vertical plane of the predicted star, something which could not be so simply mechanized with three gimbals. In general this removed the problem of gimbal lock[88] which had to be carefully avoided with a three gimbal system. This gives the C4 much greater ease of reorientation during MIRVing, when reentry vehicles are dropped off onto different trajectories.

The proponents of stellar-inertial guidance at Kearfott and elsewhere certainly saw its adoption as enhancing system accuracy. But it is important to note that the design of the Mk5 guidance system for Trident C4 did *not* unequivocally prioritize accuracy. Thus, the accelerometer chosen for the Mk5 guidance was essentially the PIPA used in Poseidon's

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86. 'Trident Missile Capabilities Advance', Aviation Week & Space Technology, (June 16, 1980), 99.
87. See 'Specified Range Forecast for Trident', Aviation Week & Space Technology, Vol. 102 (May 5, 1975), 55.
88. In a three-gimbal system an axis of sensitivity can be 'lost' when two of the gimbals become parallel, leading to a potentially disastrous failure of platform stabilization.

Mk3 system with a few modifications, 'largely things that made it more producible'.[89] It was considered by SPO to be good enough to meet the accuracy goal and light enough to meet the range goal, and so the extra cost of developing a new accelerometer was judged to be not worthwhile:

> ... we chose to stay with the accelerometer because we didn't have to go out and re-invent the thing ... Staying with the accelerometer certainly simplified the job... Inertial components ... are always difficult to do whenever you start to design some new ones. Not just the design and development, but also getting the production system up to speed. Start-up costs, start-up problems - they're always tremendous.[90]

Producibility concerns also played a role in the selection of the gyroscope for the Mk5 guidance system, where Kearfott, whose overall Unistar stellar-inertial concept had been adopted, also turned out to be successful. For the first time in the Fleet Ballistic Missile programme, Draper floated gyroscopes were abandoned, with SPO instead favouring Kearfott 'dry' tuned-rotor gyros. In this, the spinning rotor was supported on a shaft direct from the motor, in a sort of 'mushroom' or 'umbrella' set-up. This support is not rigid, and is so designed that at the rotation speed of the gyro wheel, the 'spring' effect of the support is exactly counterbalanced by a 'negative spring rate' of the rotor. This 'canceling out', tuning, effect means an effective decoupling of the gyro wheel from its support.[91]

Kearfott and also Litton, the other main supplier of inertial navigation for the military aircraft market in the United States, both developed major dry tuned-rotor gyro programmes in the 1960s. The technology was less labour-intensive than floated gyro technology for a given level of performance, and thus less expensive, at least at American wage levels. And because it was analogous to a 'free' gyro rotor, the dry-tuned device could detect rotations about two axes - it was a 'two degree-of-freedom' gyro. So only two dry tuned-rotor gyros were needed in an

89. Interview.
90. Ibid.
91. A crucial paper is Edwin W. Howe and Paul H. Savet, 'The Dynamically Tuned Free Rotor Gyro', Control Engineering, (June 1964) ,67-72.

inertial system, not three as with the one degree-of-freedom floated instrument.

Nevertheless, despite their apparent advantages, dry gyros might well not have been introduced to the FBM programme had it not been for Kearfott's 'plain flatout aggressive salesmanship'.[92] Together SPO guidance branch SP-23 and the Draper Laboratory considered the trade-offs. As guidance Design Agent for SPO, Draper Laboratory's conclusion was that either gyro approach would meet the system goals, and a Draper design would probably be more expensive though less risky.[93]

Also in Kearfott's favour was that two of their gyros would be smaller and lighter than three Draper instruments, an important consideration in C4, where weight savings were vital to stretch out the range to almost double that of the same-sized Poseidon. This made room for the stellar sensor, which because of its ability to compensate for errors elsewhere in the system, helped undercut the argument that Draper gyros were more accurate than the dry tuned-rotor design. In the end 'the Kearfott gyro was selected on the basis of producibility and cost and ... demonstrable accuracy adequate for the job'.[94]

The Mk5 guidance computer was an evolutionary development from that of Mk3, with the addition of a stellar subsystem. The stellar update computation and corrections had to be made very rapidly at the start of the post-boost or 'bus' phase, in order to maximize the amount of bus fuel left for deployment of the reentry vehicles. To do this the computer has virtually 100% throughput during the stellar update, and so this 'sizes' its computational power. The Mk5 computer has about 200K of PROM (programmable read only memory) which stores the guidance equations and steering laws, and about 48K of plated wire RAM (random access memory) for parameters read in prior to launch. Components are largely small and medium scale integration (SSI and MSI).[95]

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92. Interview.
93. Interview.
94. Interview.
95. Graydon M. Wheaton, 'Electronics Manufacturing for Inertial Guidance Systems' (9 May, 1986, typescript), 5.

The Mk5 guidance system never became as controversial in the formal political system as the cancelled Mk4 stellar-inertial option for Poseidon. Some funding was apparently cut by Congress from Trident I stellar inertial guidance funding in 1974 and 1975, but this had little impact on the programme.[96] Whereas Mk4 had been specifically touted as a hard target kill enhancing technology, the emphasis in Trident I was on longer time between navigation resets and hence greater submarine security - features quite compatible with an 'assured destruction' role.

The Mk500 'Evader' Re-entry Vehicle

What did temporarily re-ignite the hard-target controversy was an alternative reentry vehicle, the Mk500 'Evader'. Unlike all previous FBM re-entry vehicles this does not simply fly on a ballistic trajectory after being released by the bus, but can perform preselected manoeuvres once within the atmosphere. It was developed in response to a request from the Office of the Secretary of Defense in 1973:

> ...provide reasonable assurance that a possible later decision to initiate engineering development for service use of a maneuvering re-entry vehicle would not require reengineering of the Trident weapon system. ... include sufficient flight tests of an advanced development prototype MARV to demonstrate compatibility with the C4 missile and the Trident weapon system.[97]

Some at SSPO considered the Mk500 development an unnecessary waste of their time, but so long as the funding was readily forthcoming, SSPO's leadership had few qualms about it. Its official rationale was as a hedge against possible Soviet ABM developments - particularly the upgrading of surface-to-air missiles - which were the subject of disputed analyses from various parts of the intelligence community:

> To gain the increased full payload range [with the C4], it was necessary to give up some of the maximum possible ABM exchange ratio which would only be of value should the then proposed ABM treaty be abrogated. As a hedge

96. See D. Shapley, 'Arms Control as a Regulator of Military Technology', Daedalus (Winter 1980), Vol. 109 (1), 145-157 at 149.
97. DDR&E Memorandum to the Assistant Secretary of the Navy for Research and Development, February 8, 1973 quoted in SASC FY1976, Part 10 R&D, (Washington, DC: US GPO, 1972), 5358.

> against such a contingency, advanced development of a manoeuvring, evader re-entry vehicle capable of being carried by the missile was included in the program.[98]

The Mk500 comprises a bent-nosed re-entry vehicle containing a simple guidance system. This is a two gimbal design using two Litton dry-tuned gyroscopes and three Bell accelerometers. Once within the atmosphere the bent nose causes aerodynamic lift which is controlled by rolling the vehicle by shifting an internal weight (in fact the electronics package). This was a relatively rudimentary approach to the task of developing a manoeuvring re-entry vehicle, best suited for evasion, with accuracy a secondary consideration. Indeed some loss of accuracy compared to the baseline Trident Mk4 re-entry vehicle was considered acceptable, though later studies did suggest ways in which the Mk500 could be used to improve accuracy. At the time, during the mid-1970s, it was viewed by some as an attempt to gain hard-target kill capability, and provoked some opposition in Congress.[99] But as no attempts were made to deploy the Mk500, the controversy petered out.

The Mk500 has been tested eight times, with five utilising Minuteman I boosters between March 1975 and January 1976.[100] All flights were reported to be successes and the programme now seems to be completed. It demonstrated the ability to carry an evasive re-entry vehicle on Trident, though the lead-time to manufacture the technology for deployment is estimated at three and a half years.[101]

SINS and the ESG Monitor

The introduction of a star-sensor complicated the relationship between submarine navigation (the province of the branch of the SPO known as SP24) and missile guidance (SP23). From a situation of relative independence, their work became much more closely related. The star-sensor permitted a degree of *posthoc* correction of errors in the information about launch position and heading that the missile guidance

98. Smith et al, 12.
99. D. D. Dalgleish and L. Schweikart, Trident (Carbondale, Il.: Southern Illinois University Press, 1984), 91.
100. SASC FY1977, (Washington, DC: US GPO, 1976), Part 11, 6516; Part 12, 6556.
101. SASC FY1981, Part 6, 4099.

system received from navigation through the fire control system. 'Partitioning' of the task thus became more difficult. It was in any case a process inevitably affected by 'the realities of budgets and organizations, politics in the broad sense of the word - I don't mean political politics, [but] office politics, or budgets or contract capabilities'.[102] According to one account, the introduction of a fully digital link (rather than analogue 'synchros') between navigation and fire control/guidance was delayed because it was difficult to agree the form of the link. Should the navigation computer have to 'broadcast' data several times a second, whether or not that information was being used, as those responsible for missile guidance would have liked, or ought the rest of the system, as those responsible for navigation preferred, have to 'request' data from navigation, with the answer possibly being delayed?[103]

The star-sensor's ability to correct for navigation errors reduced the demands on SINS accuracy, though improvements were still sought here to achieve longer reset intervals. Despite claims from advocates of the electrostatically-supported gyroscope that 'preliminary test data indicated a quantum improvement in performance with a system using ESGs over one using conventional SINS gyroscopes',[104] SSPO proceeded conservatively. An evolutionary modification of the existing Autonetics SINS, the Mk2 Mod7, was chosen. However, the ESG did now find a place in the Trident submarine (and the Poseidon submarines retrofitted with Trident I missiles), as a 'monitor' overseeing the two SINS. The ESG Monitor did not directly provide navigational information, but was used to update the SINS periodically. The same overall accuracy of SINS output could thus be maintained, while increasing the time intervals between external resets, and thus decreasing the vulnerability of the submarine to anti-submarine warfare.

The ESG that went into FBM submarines was, however, not Honeywell's; the successful device came from Autonetics division of Rockwell International, the traditional SINS supplier. Two aspects of this

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102. Interview.

103. Ibid.

104. B. McKelvie and H. Galt, Jr. 'The Evolution of the Ship's Inertial Navigation System for the Fleet Ballistic Missile Program', Navigation: Journal of the Institute of Navigation, Vol. 25 (Fall 1978), 320.

are particularly interesting. The first is the difference between the two designs. Though Honeywell's large, hollow ball was believed by its proponents to maximize accuracy, the successful Autonetics design employed a small, solid ball, that was significantly easier to make. So 'producibility' won out over apparent theoretical accuracy. Secondly, it appears that this was exacerbated by a Honeywell management decision. At a crucial point, Honeywell shifted ESG production from their traditional site in Minneapolis to the new facility they were developing in Florida:

> We told the corporation what was going to happen to them - that they were going to move down there, half of their people weren't going to go, and this [ESG production] was an artistic thing ... They could produce them, but it wasn't something you could put on the production line. You had people who had techniques, etc... They moved down to Florida and nothing worked. Half the people didn't move, wouldn't move, some of them retired, everything we said happened with spades. Eventually they were in deep [trouble] ... [Autonetics] persuaded us that since Honeywell was falling on their face, we ought to give them a chance, and we decided that we would. And they funded much of that chance themselves, and the answer is we had a time when we wanted to see the Honeywell thing on the [USS] *Compass Island* [the ship used for testing navigational equipment], and when that time came Rockwell had the one there and knew how to run their thing and ... Honeywell delivered one a few months late and they hadn't the slightest idea what to do with it...[105]

Trident I Deployment

Once the initial development problems - especially in the propulsion and electronics areas - were overcome, the Trident I flight test programme, carried out between 1977 and 1979, was considered very successful.[106] So much so that the number of flight tests was reduced from 30 to 25.[107] But with the first Trident submarine well behind the 'promised' December 1977 delivery date, the decision to backfit Trident I into existing FBM submarines seemed to have been proved wise.

105. Interview.
106. Of the eighteen pad launches and seven submarine launches only one (14 February, 1978) is not recorded as successful in Strategic Systems Program Office, FBM Facts/Chronology - Polaris, Poseidon, Trident (Washington, DC: Navy Department, 1986),, 47-53.
107. Ibid, 51.

In retrospect it seems that many, including some at SSPO, would now question the urgency of Trident I deployment.[108] The potential advances in Soviet anti-submarine warfare do not seem to have been realized, and the extra sea-room allowed the submarines by Trident I's range increase over Poseidon has been a hedge that was not needed. At the time, however, SSPO's leadership clearly believed that Trident I deployment was an urgent matter. Although the Trident submarine programme was out of SSPO's control, the backfitting of Trident I remained its responsibility.

Because of the problems that the main shipyards were experiencing with Trident and 688 attack submarine construction, SSPO decided to take the precaution of arranging to backfit some Trident Is by alternate means. Six existing FBM submarines - following the normal procedure - were backfitted from Poseidon to Trident I during their scheduled second overhaul, starting in March 1979 with the USS *Simon Bolivar* at Portsmouth Naval Shipyard.[109] However, the other six of the submarines to be backfitted were done under 'emergency' conditions, with shifts working 24 hours to complete the backfitting at temporary pierside installations.[110] Accommodating the Trident I missiles - with their blunter noses and increased weight - required the development of a new launcher system, largely based on that used for Poseidon.[111]

Navigation was also updated with the modifed SINS (now designated Mk2 Mod7) augmented by an electrostatically suspended gyroscope monitor (ESGM). This provided a way of achieving the desired increase in reset intervals whilst maintaining the reliability of the well-

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108. Interview.

109. The submarines backfitted at Portsmouth Naval Shipyard during scheduled overhauls were Simon Bolivar (SSBN 641), Benjamin Franklin (640), George Bancroft (643), Casimir Pulaski (633), Von Steuben (632) and James Madison (627). See 'FBM Facts/Chronology', 14-15.

110. These were the Daniel Boone (629), John C. Calhoun (630), Stonewall Jackson (634), Henry L. Stimson (655), Francis Scott Key (657), and Mariano G. Vallejo (658).

111. C. A. Robinson, Jr. 'New Propellant Evaluated for Trident Second Stage', Aviation Week & Space Technology (October 13, 1975), 17; also SASC FY 1977, 6553.

tried traditional SINS and avoiding any delays due to problems with the ESGM[112]:

> The role of the ESGM is to act as a source of position fix data, internal to the submarine, that can be used to periodically reset the SINS, thereby imparting its performance capability to the SINS and in so doing, achieve the objective extension in the time interval between resets using external position fix information.[113]

Fire control was also updated with the General Electric Mk. 88 replaced by a Mk. 88 Mod2 version. This development included a new computer (known as the Trident digital control computer) to calculate the presets necessary for the missile's stellar update, but the earth gravity model used by the missile guidance system remained, as in Poseidon, based on a spherical simplification, with offsets. The last of the twelve backfitted submarines, the USS *Casmir Pulaski*, went on patrol in June 1983.

Trident I and Nuclear Strategy

With its emphasis on longer range and extended submarine navigation reset interval it would seem that Trident I simply reflected traditional concerns with maintaining the potency of the 'assured destruction' threat. But, of course, there was never a consensus that this was the only role that the US FBM force should be capable of. Just as with previous systems there were advocates of greater counterforce capability, even against hard targets. On the other hand, SSPO itself and Congress, and particularly the Senate Armed Services Committee ad hoc Subcommittee on Research and Development, were still sceptical of efforts to increase the hard target kill capability of US strategic forces.

Still, advocates of greater counterforce capability were not without influence, both within the Office of the Secretary of Defense and within the Office of the Chief of Naval Operations. C4's larger warheads were largely an outcome of their pressure for greater flexibility to hit harder

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112. According to testimony from Admiral Lyon in May 1973: 'There is a medium schedule risk associated with the ESGM, however, the navigation system is configured so that it can function without the ESGM.' SASC FY 1974, 3706.
113. McKelvie and Galt, 320.

targets than Poseidon could. Similarly OSD's accuracy goal, whilst not a strict requirement, pushed SSPO towards stellar inertial guidance and accuracy greater than SSPO might otherwise have deemed necessary. As originally conceived in Strat-X, ULMS emphasized cost-effective delivery of equivalent megatonnage in the face of a Soviet first strike and Soviet ABM defenses. But as ULMS evolved into Trident I its counterforce utility became enhanced.

Just how far this could go was limited, however, by the size constraint which had been strongly advocated by Admiral Smith. By insisting that Trident I should be small enough to be back-fitted into the existing FBM submarines, he effectively ruled out the use of very large warheads. With the missile volume available the range goal made a third stage seem unavoidable and this then left an annular space for reentry vehicles too small for the very large warheads that some would have liked to see deployed on FBMs. With regard to accuracy improvements, Admiral Smith was again cautious, claiming that it still was not possible to promise to meet a requirement for high accuracy. High accuracy could not simply be bought by building a stellar inertial guidance system. It required considerable investments in instrumentation and modeling to understand and validate what was happening, as well as very expensive improvements in other aspects of the system, such as submarine velocity knowledge and gravity and sea-bed terrain mapping.

Nevertheless, C4 appears to have turned out much more accurate than the original goal. A 1984 report claimed that 'during 1983, the Navy's tests achieved consistent 750-foot CEPs with Trident I, twice as good as the 1500-foot goal'.[114] This makes Trident I a significant threat to all but the hardest Soviet targets. But to many this intermediate capability seemed of limited value:

> There was no point in going to intermediate accuracy because it wouldn't do any good. That was the C4. The C4 got intermediate accuracy and we built a new warhead and so what? It didn't provide a capability that bridged a new set of targets. It was still only useful against soft targets

[114]. W. M. Arkin, 'Sleight of Hand with Trident II', Bulletin of the Atomic Scientists (December 1984), Vol. 40, 5-6. Many interviewees also confirmed that C4's accuracy performance had exceeded that requested.

> and pretty useless against hard targets. And it turns out that there are soft targets, then there's a small number that are slightly hard, 50 to 100 psi, and then you start going thousands of psi - bunkers, command bunkers and silos - and there's very little in the middle. You just don't waste money building systems tailored to these intermediate targets and that's what happened. C4 ended up being useful against a set of these intermediate targets that don't exist in any significant numbers. . . . if you want to use it against a hard target, you've got to use several, and when you use several you have terrible targeting problems.[115]

But by the time C4 was deployed, the pendulum would have finally swung all the way towards hard target counterforce. It would have become offically-stated US policy and an Improved Accuracy Progam, set up in 1974 at Secretary of Defense Schlesinger's instigation, would have undermined SSPO's arguments against committing themselves to a high accuracy requirement. The next FBM, Trident II, would be specifically and overtly designed to have the capability to destroy very hard targets.

[115]. Interview.

Chapter 7

The Improved Accuracy Program and Trident II

> **... if the TRIDENT submarine is now seen as a vehicle to sell a larger payload missile, rather than as a vehicle to assure the invulnerability of the sea-based offensive force, we will have lost a great deal of credibility ...**
>
> **Admiral Smith.**[1]

The ULMS 'decisions' of the early 1970s, and particularly the final design of the submarine, made the development of Trident II almost inevitable at some point. Although other factors - particularly Rickover's desire to build new large reactors and 'the Navy's' desire to justify new FBM submarines - drove the larger size of the Trident submarines, they were justified on the basis of the need to carry the large Trident II, which itself was characterized as a continuation of the Strat-X missile concept.[2] There was little doubt that a Trident II would make full use of the extra launch tube volume, but what remained to be decided was exactly when, and what capabilities the missile would possess. In the original ULMS conceived in Strat-X, long range had been considered an important attribute and originally Trident II was to have a 6000-mile range, as opposed to the 4000 miles of Trident I.[3] In the late 1960s and early 1970s extra range provided an uncontroversial way of justifying the new missile. However, by the time it came to be developed, the emphasis would have shifted from enhancing range to enhancing accuracy, an attribute which

1. Memo for OP-21, (21 March 1974), quoted in 'Admirals' Admirable Quotes: An Incomplete Collection', (October 1981, typescript). My thanks to RADM Robert Wertheim for providing me with this - presumably an in-house SSPO production.

2. Thus Admiral Isaac C Kidd Jr. (former chief of Naval Material Command) would claim that: 'The missile sized the submarine'. M. Mintz, 'Depth Charge: Cost Overruns on New Trident Sub Leave a Muddied Wake', Washington Post (October 4, 1981), reprinted in Dina Rasor (ed.), More Bucks, Less Bang: how the Pentagon Buys Ineffective Weapons (Washington, DC: Fund for Constitutional Government, 1983), 211-23, at 213. However, in testimony on May 22, 1973 (following the Trident submarine design definition) Admiral Kaufman would 'emphasize that this missile [Trident II] has not been defined'. SASC FY 1974, Part 5, R&D, (Washington, DC: US GPO, 1973), 3597.

3. Strategic Systems Program Office, A Programmatic History of Trident II (Washington, DC: SSPO, November 1982), 1.

was earlier considered not especially important by many, and even destabilizing by some.

The Improved Accuracy Program

Through the 1960s SPO had resisted pressure to meet increased accuracy 'requirements' for the FBM force. Requests from the Office of the Secretary of Defense and from the Office of the Chief of Naval Operations met a standard response, which embodied a distinction that epitomized Admiral Smith's approach. SPO would attempt to meet accuracy 'goals', but measurement and understanding of FBM inaccuracy was not good enough to promise to meet 'requirements'.

Following a 1972 request from Chief of Naval Operations, Admiral Zumwalt, SSPO Director Levering Smith estimated that he would need $1\frac{1}{2}$ billion to assure an improvement in FBM accuracy.[4] This led to SSPO asking Johns Hopkins University Applied Physics Laboratory to develop a new error model to allow better understanding of FBM test results. Then in late 1973 the new Secretary of Defense, James Schlesinger, 'asked the Chief of Naval Operations for a presentation on possible improvements of accuracy of the sea-based strategic system'.[5]

Schlesinger, like Robert McNamara in the previous decade, was an activist Secretary. An economist by training, Schlesinger had headed the strategic studies division of the RAND Corporation, and there had come to favour 'limited nuclear options' - relatively small-scale, selective nuclear targeting, designed to exert political leverage. This was also the main thrust of a review of nuclear strategy which had been conducted during 1972 and 1973, and which Schlesinger then adopted and promoted. Known as National Security Decision Memorandum 242, and signed by President Nixon in January 1974, the resultant new policy marked a radical departure from the previous *declared* policy of assured destruction. In fact, the Single Integrated Operational Plan for targeting nuclear forces did allow some relatively limited options during the 1960s, but NSDM 242

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4. SASC FY 1975 (Washington, DC: GPO, 1974), 3298.
5. Ibid, 3292.

went much further in providing preplanned options for small-scale nuclear strikes against military targets.[6]

Although Schlesinger argued that the flexibility of NSDM 242 could be achieved with the existing arsenal, he considered greater accuracy, and greater confidence in accuracy figures, to be desirable. He 'just kept pushing for improved accuracy' in the fleet ballistic missile programme.[7] As before, SSPO's leadership was unwilling to commit itself to a stringent accuracy requirement 'because they still had essentially no ability to correct for excess errors if tests of the developed system showed that the requirements had not been met. They lacked the ability to measure the magnitude of error contributions and the understanding to extrapolate errors to other than test conditions'.[8] Schlesinger was impatient with this, as Admiral Smith recalls:

> I remember a couple of sessions with him personally when I was trying to show that we were unable to explain the fall of shot. He rolled up his sleeves and said "OK, I'll explain it for you". And we sat down with the raw data a couple of hours each time.[9]

The Improved Accuracy Program emerged from these discussions. Secretary of Defense Schlesinger's Posture Statement of March 4, 1974 noted that:

> We plan to undertake an advanced development program which will define our capability to improve and measure the accuracy of our SLBMs and which, if implemented by retrofit, could lead to improved accuracy in the future.[10]

SSPO again avoided any strict requirement for accuracy improvement in Trident I, but committed itself to undertake a programme involving three broad areas of development: accuracy error model analysis, instrumentation, and component development. In January 1975 SSPO

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6. See D. Ball, Deja Vu: The Return to Counterforce in the Nixon Administration (California: Seminar on Arms Control and Foreign Policy, 1974); Interview. a Monica,

7. Interview.

8. Letter from VADM Levering Smith to Donald MacKenzie (13 October 1986).

9. Interview.

10. SASC FY 1975, 3288.

received further direction from the Director of Defense Research & Engineering to:

> Restructure the accuracy improvement program to accommodate funding adjustments and to be compatible with providing an improved accuracy capability for the Trident II missile with IOC in FY [deleted]. Incremental accuracy improvements in the Trident I missile should be pursued when cost effective.[11]

Whilst explaining the IAP in Congressional testimony Admiral Smith outlined the inadequacies of previous FBM accuracy assessment methods:

> Those methods are influenced by the fact that in the current weapons system, C-3 and C-4, accuracy is a goal. It is not stated as a requirement. We did not propose in the C-3, and we have not to this point proposed as a part of the Trident C-4 program, the funding of a high confidence assessment method.
>
> The accuracy assessment is approached basically by the direct or splash assessment, the limited subsystem error assessment, and limited modeling techniques.
>
> This has resulted in low statistical confidence because of the small number of test flights, the limited variety of operational conditions available to us, and the limited subsystem error measurement capability.[12]

The basic objectives of the IAP were to:

> Gain an understanding of SLBM error sources and their relationships. Based on this understanding, assess the accuracy improvement potential of:
>
> improved components
>
> advanced system concepts
>
> Conduct advanced development of promising:
>
> improved components
>
> advanced system concepts.[13]

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11. SASC FY 1976, 5317.
12. SASC FY 1975, 3289-90.
13. SASC FY 1977 (Washington, DC: GPO, 1976), 6640.

A major part of the IAP was the development of new instrumentation methods to provide more information about the sources of error both in the submarine position and velocity prior to launch and during the missile flight. Bottom mounted transponders in the ocean areas used for test launches provided more accurate data on the submarine's position and velocity at launch. For determining missile position and velocity during flight radar improvements at the Eastern Test Range were supplemented by a satellite tracking system, known as Satrack. Satrack emerged from the 1973 accuracy evaluation study done by the Applied Physics Laboratory for SSPO, which 'indicated that a satellite-based system could meet the major objectives of SLBM accuracy evaluation at the system flight test level'.[14] This was based on the Navstar Global Positioning System satellites, and used a similar principle - comparing time delays of signals sent to the missile from various satellites and a test ship and then retransmitted back (at a different frequency). Satrack was available in mid-1978 for the final Trident I development missile launches from Cape Canaveral and for the submarine-launched tests beginning in early 1979.

Instrumentation was accommodated on-board Trident I test missiles in the space left by replacing the warheads. When the Trident I flight tests began they were used to validate improved accuracy error models which had been developed meanwhile. At the same time, in addition to developing more sophisticated error modelling and investigating improved components, there was also consideration of a range of different ways of improving accuracy.

Three main ways of improving accuracy were considered. One was to take advantage of the emerging satellite navigation system, the Global Positioning System, which offered extremely accurate position fixes, by placing GPS receivers on missiles. However, GPS suffered from concerns about its vulnerability, both operationally in a nuclear war, and programmatically, in the battle for funding. Although all the services expected to benefit from GPS, none were especially keen to provide the

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[14]. See T. Thompson, 'Performance of the Satrack/Global Positioning System Trident I Missile Tracking System', Proceedings of IEEE 1980, Position Location & Navigation Symposium, 445-49, at 445.

funding for it and the resultant lack of a firm commitment to the system has allowed 'technical' problems to cause delays in deployment of a full satellite 'constellation'. Mid-flight updates from GPS were considered likely to provide accuracy as good as improvements to a stellar inertial system, but the potential vulnerability to countermeasures, and to the availability of GPS counted against it.[15]

The second approach to accuracy improvement was to move to 'homing' re-entry vehicles, which would use some method of electromagnetic recognition to take a precise 'fix' in the target area. This offered the 'highest accuracy potential', but it too was susceptible to countermeasures and was the 'least developed technology' of the three. A particular objection concerned its 'testability', 'our ability to conduct flight tests over land'.[16] For obvious legal and political reasons, US ballistic missile tests are conducted primarily over water, and impact is by 'splash down' in areas such as Kwajalein Atoll. A homing re-entry vehicle would have to 'recognize' terrain features, and there was thus a major question mark over whether it could be adequately tested without politically difficult overland testing.

The third way was further development of the stellar-inertial guidance technology used in C4. Generally incremental improvements there offered 'a rather significant improvement potential in accuracy on the order of [deleted] feet, CEP at the 4,000 nautical mile range. ...in order to achieve this kind of system accuracy, we are going to have to improve accuracy essentially across the board in almost all areas of the system, navigation, fire control, guidance, geodesy, and the like...'[17] Of these, the two biggest errors identified in the IAP were in submarine velocity and in the stellar sensor system.[18]

The Improved Accuracy Program ran from 1974 to 1982, and cost of the order of $600 million over the period.[19] It provided the means by which

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15. See the testimony of Rear Admiral Robert H. Wertheim, SASC FY 1979, Part 9, Research and Development (Washington, DC: GPO, 1978), 6683.
16. Ibid , 6684.
17. SASC FY 1979, 6683.
18. SASC FY 1978, 6564.
19. Interview.

the various options to improve accuracy could be assessed. Most of the funding went towards development of the stellar-inertial guidance technology - basically improving the techniques and components used in the Trident I Mk5 guidance system. Work on mid-course and terminal updates was largely restricted to 'paper studies and investigation'.[20] It was not surprising, then, that with the next FBM system, Trident II D5, it was decided to stay with stellar-inertial guidance. As so often before SSPO preferred, if possible, to deal with familiar technology and familiar organizational relationships.

Trident II D5 - Decisions

Given the survival of the Trident submarine programme, it was difficult to envisage the C4 as other than an interim missile. The much bigger submarine made possible much bigger missiles, and from the inception of the programme a second, big missile, a Trident II, was projected. During the 1970s the date for Trident II IOC was shifted around, from as early as FY 1982 to FY 1987.[21] After denying funding for Trident II initial studies in 1975 and 1976, Congress finally gave the go-ahead in 1977. The issue then was what to do with the extra volume available in a Trident submarine missile tube.

Various options were considered through the mid 1970s: C4 with better accuracy; a long C4 with a new first stage to give increased range (thus known as C5); a 'stepped' missile using an 83-inch first stage with 74-inch upper stages that retained some commonality with C4; a D5 missile with third stage protruding through the re-entry vehicles (as in C4); or a 'clear deck' D5 reverting to two stages to provide more space for the payload.[22] Out of these options the three-stage D5 was chosen to fully use the tube space available in the Trident submarines, but of all the technical characteristics, accuracy was to be paramount. For the first time hard-target kill capability became an unequivocal driver of US FBM technology development.

20. SASC FY 1978, 6544.
21. SSPO, 'Programmatic History', 2-6.
22. SASC FY 1978, 6681-2; also Programmatic History, 3.

But initially, in the early 1970s, SSPO's tentative accuracy goal for Trident II was 'to achieve at 6000 nm the CEP of POSEIDON at 2000 nm'.[23] Then came the pressures from other branches of the Navy - including PM-2 (the Trident Project Office) and OP-21 - and from the Office of Secretary of Defense which culminated in the Improved Accuracy Program. Increasingly improved accuracy came to be seen as important to provide higher counterforce capability for the FBM. In a May 1976 Memorandum to the Secretary of the Navy, Deputy Secretary of Defense, W. P. Clements, Jr., referred to 'the ability of the FBM forces to respond to the guidance provided by NSDM 242 and the NUWEP'. Along with the relative invulnerability of the submarine, he noted:

> the potential for increased throw weight in a follow-on to the Trident I missile, encourages consideration of options to expand our SLBM capability against the full spectrum of the target system. Towards this objective, improvements in communications and in payload, including [deleted] and weapon system accuracy in a follow-on to the Trident I missile would enhance the utility of the FBM weapon system. . . . It is therefore requested that the Navy develop an overall plan, including a plan for the development of a Trident II missile with an IOC in the 1980s, for increasing the utility of the FBM weapon system. Increasing SLBM throw weight should not be pursued as an objective independent of substantial accuracy improvement.[24]

Such pressure for accuracy improvements in the FBM system was not new, of course. Since at least the early 1960s there had been pressure from the Office of the Secretary of Defense to increase FBM hard-target capability via accuracy improvements. But such pressure had only produced a grudging response. Accuracy improvements were made, but did not receive the highest priority, and hard-target capability did not increase significantly. Throughout the 1960s and 1970s the FBM force remained differentiated from the Air Force ICBMs, in both perceived capability and in doctrinal attitudes. Whereas counterforce was the byword of Air Force planning, the Navy remained wedded to deterrence by retaliation. But by 1976 things were beginning to change - future FBM needs were now considered to include the ability to 'strike hard targets to hedge against dependence on ICBM's'.[25]

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23. Ibid, 2.
24. Quoted in SASC FY 1978, 6570.
25. SASC FY 1977, 6532.

This represented a significant change. Not only would hard-target kill capability come to be a central feature of FBM design, but it would do so as a clearly perceived substitute for Air Force ICBM hard-target kill capability. But this did not, it seems, come about due to the efforts of those Navy strategic planners who had long wanted to challenge Air Force dominance of the counterforce missions, nor simply because the technology was now available. Indeed Trident II came to be seen as a substitute for the Air Force MX ICBM by default, because of difficulties which threatened that programme, rather than because of advocacy by the Navy.

What happened was that the various strands of US nuclear policy came together to form a powerful consensus around the desirability of US possession of significant hard-target kill capability, but during the same period in the late 1970s it became evident to many that MX might not be able to satisfy this 'requirement'. The reasons for this highlight just how broadly 'technical' issues must be understood.

The Shift to Counterforce

Counterforce, and the targeting of 'hard' targets, are not recent themes in the nuclear arms race. Since at least the start of the 1950s a significant portion of US nuclear weapons were assigned to 'the destruction of known targets affecting the Soviet capability to deliver atomic bombs', which initially were designated as Bravo (for blunting) targets.[26] With the advent of nuclear-armed ballistic missiles, despite their initially poor accuracy, the theme continued. In 1957 such targets were above ground and only considered able to withstand 100 pounds per square inch overpressure. Comparing the prospective US missile force, the Pentagon's Weapon System Evaluation Group concluded that: 'The numbers of successful missiles required to achieve 50 per cent probability of destruction of such a target are 80 ICBMs, 26 POLARIS's, 13 THOR's, and 2 JUPITER's.'[27] The study seemed intended to show that ballistic missiles

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[26]. Quoted in D. A. Rosenberg, 'The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960', International Security, (1983), 3-71, at 17.
[27]. Weapon System Evaluation Group Report No. 23, 'The Relative Military Advantages of Missiles and Manned Aircraft' (May 6, 1957), 27 and 22.

were not suitable for counterforce missions (thus supporting continued reliance on bombers)[28], and did not consider Polaris 'suited for employment against 100 psi targets'. But it was not to be long before the Air Force would come to emphasize counterforce in its ICBMs too (starting with Titan II and Minuteman II), and pressure would develop for the Navy to compete over the role.

Yet pressure for a counterforce FBM remained localized until the 1970s. Although initially drawn to counterforce for damage-limitation, Secretary of Defense MacNamara soon switched his public, declaratory stance towards 'assured destruction'. Although the targeting plan, the SIOP, remained based on the earlier counterforce doctrine,[29] nuclear policy came to be publicly justified, and 'sold' to Congress, on the basis of retaliation against urban-industrial targets. Early attempts to fund accuracy enhancements explicitly to provide hard-target kill capability were not then well-received.

But as the Soviet Union achieved rough numerical parity in strategic forces with the US, a parity enshrined in the 1972 SALT Treaty, attention shifted increasingly to the quality of the two arsenals. Although defence liberals continued to argue that US counterforce capability reduced US security (by potentially placing the Soviet Union in a 'lose 'em or use 'em' situation), hawks pointed to the daunting counterforce capability possessed by the Soviet Union's 'heavy' ICBMs. True, the Soviet Union could not hope completely to disarm the United States, but what would happen if it could successfully destroy the only US counterforce-capable missiles, the ICBMs? Would a US President not then be forced to surrender, given no option other than a suicidal attack on Soviet cities?[30]

This 'second-strike counterforce' argument undercut opposition to counterforce without violating the liberal sentiment that the US should never be, and should never even threaten to be, the nuclear aggressor. To the right of it, however, was to be found a more explicitly hawkish analysis, that suggested that numerical parity should not dissuade the US

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28. See Chapter 3 of forthcoming book by Donald MacKenzie,

29. See D. Ball, Targeting for Strategic Deterrence (London: International Institute for Strategic Studies, Adelphi Paper #185, 1983), Vol. 25

30. P. Nitze, 'Deterring our Deterrent', Foreign Policy (Winter 1976/1977), 195–210.

from the pursuit of 'nuclear superiority' and the political leverage that might follow from it.[31]

All this added up to a climate gradually pushing US official nuclear strategy ('stated posture') towards counterforce. Schlesinger's NSDM 242 of 1974 stopped short of clearly calling for enhanced counterforce capability, but it started a trend. President Carter's 1980 'Presidential Directive 59' demonstrated how far the domestic and international political climate - especially the Presidential challenge from Reagan - could push towards counterforce a President whose original inclinations were strongly towards a minimum deterrent 'assured destruction' strategy.[32] Under Reagan, of course, Executive sympathies turned entirely against assured destruction, and towards both counterforce and active anti-missile defences.

This shift in public position certainly had its consequences, for example undercutting the possibilities for Congressional opponents of counterforce to argue - as they had been able to before 1974 - that hard target kill capability was incompatible with US national strategy. Yet the significance of this shift should not be overstated. 'Stated posture' is only one 'level' of nuclear policy: targeting practice, and acquisitions policy, by no means always follow stated posture. During the heyday of 'assured destruction' there was always a strong lobby, especially in the the Air Force, for counterforce. Counterforce capability was an important determinant of the design of the second two of the three generations of the Minuteman ICBM force, and counterforce targets received high priority in the targeting plan for nuclear war, the SIOP (Single Integrated Operational Plan).[33]

The MX Relationship

Here, however, the second aspect of the 'environment' of Trident D5 development becomes important, its relationship to the proposed new Air Force ICBM, MX. MX's Air Force proponents saw its main virtue as

31. See, for example, Colin Gray, 'Nuclear Strategy: The Case for a Theory of Victory', International Security Vol 4 (Summer 1979), 54-87.
32. See Thomas Powers, 'Choosing a Strategy for World War III', Atlantic (November 1982), 82-110.
33. Ball, 'Targeting'.

being its dramatic enhancement of US counterforce capability. But - in part at least because of Congressional sentiments - the MX programme was not put forward primarily on these grounds. Instead the main public argument for MX was what became known as the 'window of vulnerability' argument: that growing Soviet counterforce capability threatened the Minuteman force in its fixed silos.

This argument proved to be a double-edged sword. While it increased the acceptability of MX in a climate only gradually moving towards approval of the overt pursuit of hard-target kill capability, it gave high salience to finding a basing mode for MX that would be seen as invulnerable. This proved the Achilles heel of the MX programme. Successive proposals ran into both 'political' and' technical' difficulties, and the repeated failure to find an acceptable basing mode began to threaten the MX programme as a whole.[34] Paradoxically, this built support for a 'hard-target' Trident on both the 'right' and the 'left'. Advocates of increasing US hard-target kill capability realized the importance of Trident II as a hedge against non-deployment of MX. Dr Seymour Zeiberg, Deputy Undersecretary of Defense for Strategic and Space Systems in the Carter Administration, and a proponent of MX, noted the relationship:

> If we move out with a vigorous MPS [Multiple Protective Shelter basing mode for MX] program and we buy a new strategic capability which has high accuracy and has the potential to cope with counterforce missions, certainly the urgency to move out with the Trident II for that reason diminishes ... If we don't have an accelerated MX program of that sort, we would endorse the very accelerated Trident II program.[35]

For this reason, Zeiberg pushed SSPO to see if they could achieve accuracy in Trident II that was comparable with that forecast for the MX.[36]

Congressional 'doves', on the other hand, saw MX as the main enemy. Although many opposed it because of its increased counterforce capability, they made a tactical decision to fight it on the basing issue,

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34. J. Edwards, Superweapon: The Making of MX (New York: Norton, 1982); H. Scoville, Jr., MX: Prescription for Disaster (Cambridge, Mass.: MIT Press, 1981).
35. Quoted in J. S. Wit, 'American SLBM: Counterforce Options and Strategic Implications', Survival Vol. 24 (1982), 163-74 at 168.
36. Interview.

where opposition was greatest. This then undercut opposition to Trident II which was 'sold' on its invulnerability. Because of this, and the FBM's enduring image as a retaliatory deterrent, many defense liberals saw Trident as the lesser of two evils at a time when it was politically difficult to oppose both outright.

So as the design decisions for the D5 were being made in the late 1970s and early 1980s (funding for development of the D5 was announced by the Reagan Administration in October 1981, and full-scale engineering started in 1983, although key decisions were effectively made well before that), the programme's wider environment was such as to make any internal opposition to counterforce difficult. Furthermore, Admiral Levering Smith, seen by many as a formidable opponent of counterforce, retired as SSPO Director in November 1977.

With MX in trouble, the need for careful 'differentiation' of the FBM's nature and mission from those of ICBMs diminished. The 'bureaucratic' logic for opposition to counterforce disappeared. It was still seen as prudent not to present Trident II as a complete alternative to MX - if only because the slowness of communications with submerged submarines limited the extent to which Trident could be used in a 'first strike' or sophisticated 'war fighting' mode - but direct comparisons between the two appeared for the first time. In 1983, for example, Chief of Naval Operations Admiral James Watkins testified that:

> By 1991, we believe you could have four to five D-5 equipped Trident submarines, which is more than the equivalent of an MX field in terms of hard target kill capability.[37]

Design for Counterforce

Counterforce capability thus became an overt requirement in the design process of Trident D5. Nowhere was this change in roles, and changed relationship to Air Force programmes, more marked than in warhead design. Originally the Navy had considered using a modification

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[37]. Quoted in W. M. Arkin, 'Sleight of Hand with Trident II', Bulletin of the Atomic Scientists (December 1984), Vol. 40, 5.

of the Air Force Minuteman III Mk12A re-entry vehicle with its 335 kiloton W78 warhead. This is essentially what the Mk21 re-entry vehicle and W87 warhead, chosen for the MX, are. The Air Force had intended to use a 500 kiloton warhead for MX, but 'lack of oralloy [enriched uranium] ... forced the Pentagon to opt for a warhead that uses less oralloy but which only had a yield of 300 kilotons'.[38] Air Force disappointment turned to annoyance when the Department of Defense then persuaded the Navy that Trident II should have a larger warhead to give it greater hard target kill capability and 'added $88 million to the Navy's Fiscal 1984 budget request to develop a new ballistic re-entry vehicle' for the D5.[39] The new re-entry vehicle, the Mk5, would carry a higher yield version of the MX W87 warhead, boosted to 475 kilotons by adding more oralloy. For the first time, a Navy missile was to carry larger yield warheads than its Air Force counterpart:

> Questions are being raised by the [Air Force] over why the Navy will be allowed to deploy the higher yield device requiring more oralloy in short supply in the inventory.[40]

In addition the new Mk5 re-entry vehicle - like the Mk4 an ablative design - incorporates a shape stable nose tip intended to reduce dispersion caused by uneven erosion. The carbon-carbon weave is supplemented by metal filaments running along the axis of symmetry which make the shape caused by ablation more predictable, and thus more amenable to compensation.[41] This provides more assurance that unfavourable local weather conditions, such as rain or snow, will not greatly reduce accuracy.

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38. 'Administration official' quoted in Clarence A. Robinson, Jr.,'Congress Questioning Viability of MX ICBM', Aviation Week and Space Technology (March 22, 1982), 18-20 at 19. Highly enriched uranium (at least 93.5% U-235) acquired the codename Oak Ridge Alloy or Oralloy during the Manhatten Project. The stockpile of oralloy has remained roughly constant since the early 1960s when production was stopped as more efficient weapons design and smaller warheads reduced demand. The recent trend to larger warheads and the large numbers of planned cruise missile warheads has again led to a perceived scarcity. Yield can be boosted in modern warheads by replacing other materials, such as the depleted uranium casing, with oralloy. See T. B. Cochran, W.M. Arkin, Robert S. Norris and M. M. Hoenig, Nuclear Weapons Databook Vol. II, US Nuclear Warhead Production (Cambridge, Mass.: Ballinger,1987).

39. Anon., 'Navy to Develop New Trident Warhead', Aviation Week and Space Technology Vol. 118 (January 17 1983), 26.

40. Ibid.

41. Interview.

The Mk5 is being manufactured by GEC, making it the first FBM re-entry vehicle not to be manufactured by Lockheed.[42]

Both the design of the re-entry vehicle and the yield of the warhead reflect the emphasis placed on hard target kill capability in Trident II. However, in the belief that only a part of the Navy's warheads would be allocated to hard targets, SSPO also has retained the 'flexibility' to carry the C4's 100 kiloton Mk4 re-entry vehicle, and so D5 is designed to be compatible with both. Trident II design specifications also required the 'bus' to be compatible with a future Large Accurate Evader warhead, though no such system has been developed.

The Mk6 Guidance System for Trident II D5

The push for hard-target kill capability was the central factor affecting design of the Mk6 guidance system for the Trident D5. For the first time, a particular level of accuracy was not simply a 'goal' (which could implicitly be 'traded-off' against other goals) but a 'requirement' that had to be met. And it was a demanding requirement:

> They went from 3 PIGAs to 1 PIGA to 0 PIGAs as they went through the early generations, and then of course, now they decided, 'hey, we're going to go for broke', and now they're back talking PIGAs again.[43]

The accuracy requirement was considered by those involved to be close to the limits of the possible using an evolutionary development of the C4 system:

> In our case, case of D5, I'd say we have ... an objective, a requirement in this case ... such that I'm doing just about everything I know how to do with that technology. Cost hasn't been a major consideration. ...With the basic

42. GEC manufactured the Mk12A for Minuteman III and had hoped this would be chosen for MX. When instead Avco were chosen to develop the Advanced Ballistic Re-entry Vehicle (ABRV), which was the forerunner of the chosen Mk21, it looked like GEC might be squeezed out of re-entry vehicle work. This may be what Richard DeLauer, Under Secretary of Defense for Research and Engineering, meant when he referred to 'a problem with the industrial base' with respect to re-entry vehicles. Aerospace Daily (June 18, 1982), 268. Also Interview. Apparently the solution was to give GEC the Trident II Mk5 as Lockheed would still retain Mk4 production.
43. Interview.

technology right now I'm not sure I'd know what else to do ...[44]

The lessons learnt from the Improved Accuracy Program were put to use in the design of Trident D5 guidance: indeed it was that program that gave SSPO leadership the confidence to take on an explicit and demanding accuracy requirement. The Program had led to a sophisticated and largely, though not entirely, consensual understanding of the sources of FBM inaccuracy.

It was, for example, agreed that absolute accuracy in the gyroscopes was not *per se* crucial. Sophisticated computer programs along with the star-sighting could compensate for gyro drift. One possible challenger to the existing Mk5 system's two degree-of-freedom dry tuned-rotor gyro was the laser gyroscope. However, these looked to be larger than the dry tuned-rotor design and more difficult to integrate with the stellar sensor. So it was decided to go for a two degree-of-freedom dry tuned-rotor instrument, either built by Kearfott or Litton. Kearfott's experience with the gyroscopes for the C4 missile probably decided the issue in their favour.[45]

With the accelerometers, on the other hand, the Improved Accuracy Program was understood to have shown that acceleration sensing errors were important contributors to inaccuracy. Two candidates appeared to offer the required high performance. One was the vibrating beam accelerometer whose simplicity promised small size, easy manufacture and cheapness. However, at the time it was considered difficult to harden against the effects of radiation, and so SPO reverted to the type of accelerometer used in the original Polaris, the PIGA, believed at the Draper Laboratory to be the most highly accurate accelerometer design.[46] Although not used in the Mk3 and Mk5 FBM guidance systems, the Draper Laboratory had continued to develop PIGAs, culminating in the 16-PIGA used in MX. For the Mk6 a smaller version of this, the 10-PIGA, was used.[47]

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44. Interview.
45. Interview.
46. Interview.
47. On the Mk6 accelerometer decision, see Anon., 'Trident Missile Capabilities Advance', Aviation Week and Space Technology Vol.112 (16 June 1980), 91-100, at 99.

The other main area of change in guidance components was the stellar sensor, and here 'trade-offs' continued longest into Trident II development.[48] Some argued that the vidicon technology was obsolete and that better performance could be achieved by moving to a solid state sensor, either a charge coupled device (CCD) or charge injection device (CID). Others felt these new technologies too premature for incorporation into the baseline system, and it was even felt that the reason for shifting away from the vidicon, where Kearfott had formidable expertise, was that it would allow the Draper Laboratory to regain design authority lost with the decision to move from inertial to stellar-inertial.[49] In the end CCD was selected.

Perhaps most thorough-going was the change in the guidance computer and electronics. In the chosen 'all-digital architecture' direct digital read-off from the inertial components is obtained. This provides more information about their performance and is believed to allow greater compensation for drift. Computer capacity has risen to 1 megabyte of PROM and 200 kilobyte plated wire RAM, with widespread use of VLSI (very large scale integration) components and microprocessors.[50]

Trident Navigation

All this, however, was understood as not enough to meet the accuracy requirement without improvements in navigation. With a stellar sensor believed capable of correcting for initial position and azimuth errors, two other aspects of launch condition were identified in the IAP as prominent error contributors. First, errors in knowledge of initial velocity were understood as not correctable by star sighting and so measuring the submarine's velocity was seen as critical. Various approaches to this problem were considered and a Doppler Sonar system was chosen to measure velocity from ocean bottom reflections.

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48. Interview.

49. Interview. For the 'politics' of the shift to CCDs in another context, see Robert W. Smith and Joseph N. Tatarewicz, 'Replacing a Technology: the Large Space Telescope and CCDs', Proceedings of the IEEE, Vol. 73 (1985), 1221-35.

50. Graydon M. Wheaton, 'Electronics Manufacturing for Inertial Guidance Systems' (9 May, 1986, typescript), 5, 15.

The other concern was initial misalignment in the verticality of the missile guidance platform due to local gravity anomalies. Since an inertial component cannot distinguish inertial from gravitational acceleration, the accuracy of inertial navigation depends on the accuracy of the gravity model used. For the level of accuracy desired in D5, local gravity variation could introduce significant errors into the inertial measurements. One way to reduce this error source was to develop an on-board gravity sensor system (GSS) for the submarine. This consists of 'a stabilized platform containing a gravity gradiometer and a gravimeter. The gradiometer measures the spatial rate of change of the gravity vector, and the gravimeter measures its magnitude'.[51] By constantly monitoring local gravity anomalies the GSS can help to reduce many errors which would otherwise accumulate in the navigation system and be transferred to the missile guidance system.

Another approach to the gravity problem was more accurate geodetic mapping, both by satellite and by survey ship. Gravitational data provided by previous satellites, initially Transit and then the more sophisticated GEOS III and Seasat systems was judged insufficient for Trident II and a new Geosat satellite was developed for 1983 launching:

> The Navy believes the improved Earth gravity models expected from the Geosat spacecraft will provide up to a 10% improvement in circular error target accuracy for certain Trident 2 launch areas. The Geosat data will be most useful for Trident submarine patrol areas in the southern hemisphere and parts of the Northern Pacific where gravitational survey data are limited.[52]

[51]. T. A. King and H. Strell, 'Underwater Navigation' entry in McGraw-Hill Encyclopedia of Science and Technology ,(New York: McGraw-Hill Publishing Company, 1982), 399-402, at 401. A key role in developing the gradiometer as an adjunct to inertial navigation was played by Milton Trageser at the Instrumentation Laboratory. From 1966 onwards work at the Laboratory was funded first by the Air Force, and then from about 1977 by SSPO. However, the gradiometer actually selected for Trident is not Trageser's spherical floated design. That was perceived as a 'gold plated' device - literally as well as metaphorically, in this case, since it incorporated silver-filled proof masses, gold electrodes and 'gold plating is used extensively to minimize radiant heat transfer'. See M. B. Trageser, 'Floated Gravity Gradiometer', IEEE Transactions on Aerospace and Electronic Systems ,Vol. 20 (1984), 417-19, at 419. Instead a completely different design built by Bell Aerospace Textron in Buffalo was chosen, basically because it was cheaper and easier to produce.

[52]. Anon., 'Geosat Data to Aid Trident 2 Accuracy', Aviation Week and Space Technology (19 July 1982), 26.

A new ship surveying program was also initiated, similar to that carried out for Polaris navigation, but mapping not only sea-bed terrain features, but also local gravity. Where available these surveys provide the most accurate method of updating the navigation system whilst avoiding the need to approach the surface. However, such surveying is very expensive and time-consuming, and so it was seen as impossible to survey all the potential patrol areas for a missile with the physical range of D5. SSPO thus accepted the accuracy requirement for the D5 only for a restricted range. Although D5 is capable of considerably longer range than C4 its accuracy specification was thus set for the same nominal range of 4000 nautical miles.[53]

The other main change in navigation for submarines carrying Trident II missiles will be the replacement of the traditional SINS with electrostatically-suspended gyroscope systems, no longer merely as 'monitors', but as the full navigators. However, receivers for the traditional external navigation updates, Loran-C and Transit, will be retained. Although offering potentially greater accuracy than Transit, Global Positioning System receivers will only be 'incorporated into the Trident II weapon system after GPS has demonstrated continuous, worldwide capability equal to or better than Transit'.[54]

Missile and Launcher Technology

Given the large volume available in the Trident submarine launch tube and diminishing returns of range above about 4000-miles, there was not as much pressure to improve propulsion technology in D5 as there had been in C4. Instead D5 missile design was considered 'conservative' with the emphasis on dependability and improving 'producability' to 'reduce repetitive production cost'.[55] The propellant used is Nitrate Ester Plasticized Polyethylene Glycol which took advantage of work done in the back-up investigation studies initiated because of the Trident I detonations.

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53. Interview.
54. Anon., 'Navstar offers "little improvement" in SLBM accuracy', Aerospace Daily Vol. 123, No. 33 (19 October 1983), 257.
55. Interview.

Those problems also stimulated a change in the missile case material on all but the third stage, which remained Kevlar-based. For the first and second stages, it was decided to move to graphite epoxy cases:

> The decision to go graphite case . . . was strongly influenced by our Trident I experience and the knowledge that graphite cases at the same specific strength level degrade more gracefully than Kevlar cases.[56]

Again the 'joint venture' of Hercules and Morton-Thiokol won the major part of the propulsion work - becoming subcontractors for the first and second stage. This time, however, the third stage went to United Technologies Corporation who had unsuccessfully competed for the same work for C4. After their lack of success in the competition for C3 and C4 work, Aerojet - the propellant subcontractor for the first Polaris - did not even bother to tender for D5. Other aspects of missile construction, such as the nozzle design and the use of the Generalized Energy Management System (GEMS) to avoid thrust termination, were based on the C4 technology.

Changes in missile electronics in D5 also drew on the C4 experience, as well as utilizing the latest generation of large scale integrated (LSI) chips. Without the strict space and weight constraints of C4 it was decided to abandon some of the more specialized electronics developed for C4 so as to reduce the potential supply difficulties involved.[57]

Launcher technology is also largely based on that used in previous systems, but obviously on a larger scale. The particular combination of D5's large size and its blunt nose design has, however, resulted in one significant change. All previous launcher systems have used fixed energy ejection systems, based on either compressed air or solid propellant gas generators, which were considered satisfactory for the desired launch depth band. At the shallow end of this band the missile emerged from the water surface faster than at the deep end, but both extremes were within the tolerances set. With Trident II the missile characteristics were

56. Interview.
57. Interview.

considered such that a fixed energy eject system would not provide the same launch depth band as previously without overly stressing the missile during launch. In particular, if there was to be sufficient energy to launch at the deep end of the launch band then the missile would come out too quickly at the shallow end.[58]

The solution was to devise a variable energy eject system in which the energy imparted to the missile was adjusted according to the depth of launch by correspondingly varying the amount of water added in the stand-pipe. Thus by adjusting the amount of energy used in vaporizing water it is possible to impart different amounts of energy to the missile from a fixed energy solid propellant gas generator. This means some variation in the temperature of the steam/gas mixture, but this is kept within the tolerance of the missile. It also means that the Trident II launch system is the first to require a sophisticated computer, in order to adjust the amount of water released into the stand-pipe.

A 'Non-Controversial' Programme?

The first Trident II test flight was on January 15, 1987 from a Cape Canaveral launch pad. Deployment is scheduled for 1989 on the ninth Trident submarine. It is also planned that the first eight Trident submarines - currently equipped with C4 missiles - will eventually be converted to D5. At present a total of at least twenty Trident submarines are expected to be built.

Trident II is expected to meet its stringent accuracy requirement - to be almost as good as MX and 'at least twice as accurate as the Trident I'.[59] Although with the flexibility to cover a 'full target spectrum', D5 is a system optimized at considerable expense for attacking hardened military targets, such as missile silos.

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58. Interview. The potential problem that caused concern with shallow launching was cavitation around the missile nose, that is the formation of air bubbles which on breaking down could impose excessive stress on the missile at the launch velocities inevitable with a fixed energy system.
59. SASC FY 1985 (Washington, DC: GPO, 1984), 3426.

Because of the shift in the political climate in the US, the peculiar relationship of Trident to MX, and of the FBM system's traditional reputation as a 'good' deterrent, this change in the nature of the FBM programme has been achieved remarkably smoothly. With MX still bogged down - now by a 'scandal' surrounding guidance system production as well as by its other handicaps - and the other Air Force ballistic missile programme, the Small ICBM, threatened with cancellation, Trident II was selected by Congress in November 1987 as a 'non-controversial' programme that could receive, instead of the usual annual funding, a five-year authorization.[60] The 4,000 protesters who demonstrated at Cape Canaveral in Florida in January 1987 against the first flight-test of the D5, or the 700 still demonstrating in October 1987, would not have agreed.[61] But unlike the case of MX - where relatively local 'environmental' protest in Utah and Nevada became a major cause of the programme's troubles - Trident's opponents have so far had little impact. Even the December 1987 summit meeting caused only a one day delay in the D5's test programme, as a scheduled flight-test was postponed to avoid the period when Gorbachev was in the US. Trident II seems set to enter the US nuclear arsenal as planned in 1989.

The opposition to Trident II - though limited and belated in nature - stems especially from its central characteristic as a system designed to provide a high probability of destroying hardened targets. Although most opposition to hard-target counterforce systems focused on the more vulnerable MX (land-basing making it more vulnerable to both Soviet warheads and domestic opposition), Trident II also provoked similar concerns.[62] Opposition to Trident II became more focused in the early 1980s as the implications of the system became more widely understood.[63]

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[60]. Michael Mecham, 'Congress Favours Conventional Defense, Production Efficiency', Aviation Week and Space Technology Vol.127 (23 November 1987), 23.

[61]. Anon., 'Seven Arrested at Trident Protest', Aviation Week and Space Technology Vol. 127 (2 November 1987), 27.

[62]. An important campaigner against Trident II is the former Lockheed FBM engineer Robert C. Aldridge, author of First Strike! The Pentagon's Strategy for Nuclear War (London: Pluto Press, 1983). The organization which campaigned most actively against Trident II was the Washington-based Coalition For a New Foreign and Military Policy.

[63]. See Philip M. Boffey, 'Trident's Technology May Make It a Potent Rival to Land-Based Missiles', New York Times (13 July 1982), C-1. Senator William Proxmire had the article reprinted in the Congressional Record on the same day.

Opposition to Trident II in the formal political system centred on Representative Thomas Markey – initially opposing Trident II outright, and then, when it became clear that the programme would go ahead, opposing the larger W88 warhead. The underlying rationale of this opposition was that Trident II was a 'first strike' weapon, which, irrespective of US intentions, would engender Soviet fear of pre-emption and so increase the risk of nuclear war starting in times of extreme international tension. Thus in proposing an amendment of the Defense authorization bill that would delete W88 funding, Markey argued:

> . . . do we really want to deploy this missile with a highly destabilizing first-strike capability, or do we basically want it to be a retaliatory weapon?
>
> If we were to deploy the D-5 with the lower yield warhead, it would still be able to destroy a wide range of Soviet military and industrial targets, but it would not be able to threaten a disarming first strike.
>
> Since we all know that there is no point in destroying empty Soviet silos, acquiring such a capability is useless unless we intend to strike first.
>
> And it is against U.S. policy to strike first.
>
> I say that if we are going to go ahead with the D-5, we should return the missile to its original purpose - increasing the range, and therefore the survivability, of the U.S. sea-based missile force.
>
> We should not deploy it as a silobuster.[64]

Despite the endorsement of efforts to enhance 'strategic stability' in the 1983 Report of the Scowcroft Commission on Strategic Forces, the opposition to Trident II was ineffectual. Deep in the conservative Reagan years, and with Soviet-American relations at a low ebb, it proved impossible to rally opposition to both MX and Trident II. Not only did the MX suffer from 'basing mode' vulnerability, but it also seemed more directly orientated to 'first strike'.

It could thus be argued that Trident II is much less destabilizing that MX because it is much less vulnerable.[65] In addition there is a marked contrast between the rapidity of communications of land-based and

64. Congressional Record (July 11, 1985), Extension of Remarks, E 3227-28. In the event Markey was unable to offer the amendment 'due to the press for adjournment for the 4th of July recess'! In following years a similar amendment was defeated.

65. See Theodore A Postol, 'The Trident and Strategic Stability', Oceanus Vol.28 (Summer 1985), 45-53.

submarine-based missiles. Many associated with the FBM programme dispute the notion that Trident II can be characterized as 'first strike', not only because they deny that that is its intended purpose, but also because they claim that the communications systems are inadequate for such a purpose. ELF might provide greater assurance than higher frequency systems that all submarines would receive their intended emergency action messages (EAMs), but it is still doubtful that pre-emptive strikes could be contemplated without effective, prompt, *two-way* communications. With such high stakes it would be crucial to know the availability of every missile prior to launch. Blue-green laser submarine communications technology has been under development for some time and has been claimed to herald the eventual availability of rapid, two-way communications.[66]

At present, however, the mission of the FBM force appears ambivalent. Trident II is designed to provide hard-target kill capability and has provoked concerns about its destabilizing, first strike potential. Indeed there were people working in the FBM programme who said they would never work on a hard-target FBM, and of those, some left with the advent of Trident II.[67] But if the communications are as slow and patchy as some say, then for all its accuracy and explosive yield, Trident II may not provide the capability it was intended to. Dr Seymour Zeiberg, a key advocate of Trident II in the Carter Administration, argued the case for a hard-target FBM in the warfighting language which was codified as national policy in PD-59:

> There are many targets in the Soviet Union that need to be attacked on a short time scale because they represent critical Soviet assets . . . We need to stress . . our ability to take out time-urgent Soviet targets.[68]

But whether FBM communications are responsive and flexible enough to enable Trident II to used in such a warfighting role during a nuclear conflict would seem to be open to question.

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66. See, for example, R. J. Starkey, 'The Renaissance in Submarine Communications - Part V: Blue-Green Laser/Satellite Technology: The Search for the Rainbow', Military Electronics/Countermeasures (March 1981), 48-54.
67. Interview.
68. Quoted in Edgar Ulsamer, 'Toward a New World Strategy', Air Force Magazine (January 1979), 60-65, at 61.

Thus although Trident II looks set to be a successful programme, the change in mission leaves the FBM programme with potential vulnerabilities. The FBM's traditional role, that of a survivable, last-resort retaliatory deterrent, was easy to understand, plausible in implementation, and widely and bipartisanly supported. In contrast the hard-target, time-urgent 'warfighting' role of Trident II is both difficult to understand and to implement. Even those who fully comprehend the rationale behind recent US nuclear strategy would admit it is esoteric; though others find it misguided and dangerous. Moreover, the ability to implement such a doctrine is not easily obtained, requiring more than simply high accuracy and yield. In this sense, Trident II not only marks a shift away from the traditional FBM mission, but also the end of an era for the SPO/SSPO. Lacking the autonomy of earlier years, SSPO must now find its way in a world of shifting nuclear hyperbole, with SDI the latest distraction. The only certainties in the years to come, at least until the next century, will be the size of the Trident submarine mount tubes and the limits they impose on 'the next generation'.

Chapter 8

Understanding Technical Change in Weaponry

A central question in the study of technology - and one that has particular urgency in the case of nuclear weapons technology - is how to explain change. Understanding the processes of technical change may have general utility in aiding our ability to shape technology to maximize human well-being. In most cases, of course, such a formulation is naïve - all too often one person's well-being is at the expense of another's[1] - but with nuclear weapons the issue seems quite clear-cut. Preventing nuclear war is an all-important goal for the human race, and one towards which studies of nuclear weapons technology should be able to contribute.

The threat of nuclear war deserves this central focus because of the expected enormity and widespread nature of its consequences. But understanding nuclear weapons technology also has everyday importance, though of a less unique nature. The opportunity costs of developing and building nuclear weapons are considerable, whatever the possible alternative uses of resources. Understanding how weapons technology 'decisions' come about, and how resources come to be allocated is thus of interest, both to those who wish to improve defence procurement efficiency and to those who would rather devote the resources elsewhere.

As already outlined in Chapter 1, technical change has been characterized in various ways that form a continuum ranging from the 'hard' technological determinism of 'technology-out-of-control' at one extreme to simple tool-use metaphors of 'politics-in-command' at the other. The validity of these various positions can be examined by considering their utility for explaining the development of FBM technology.

The first position, 'technology-out-of-control', considers 'hard' technological determinism - the idea that technology has an internal

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1. Some of the problems with maximising the public good - in the context of science policy - are discussed in B. Barnes, About Science (Oxford: Basil Blackwell, 1985), 124-32.

momentum and that it in turn shapes society. Each following viewpoint then casts a wider net, progressively incorporating the social world into 'internal' explanations of weapons development. First, 'soft' versions of technological determinism - such as 'technology creep' - are assessed. Then the idea that it is not the technology as such, but rather the technologists that are out-of-control. The last set of possible causes of a 'technological imperative' considers a still wider set of domestic actors, looking at the possibility that such an apparent effect might be due to an institutionalized 'internal' arms race or to the vested interests of a Military-Industrial complex. Then, at the opposite end of the spectrum of views, explanations based on 'politics-in-command' are examined. Here weapons developments are considered to be the result of 'external' factors, such as the Soviet 'threat', which are rationally analysed to produce a response - a classic case being known as the 'action-reaction' phenomenon. The role of nuclear strategy - both declared doctrine and the operational targeting plan - as a determinant of technology is also considered. Finally, there is the 'bureaucratic politics' approach - a 'middle' position in the spectrum of views, which seems to allow the combination of 'internal' and 'external' explanations, and to provide a more realistic model of 'decision' making.

Technology-out-of-control?

It has long been argued that nuclear weapons technology advances because of some internal momentum. Many authors have claimed, as Ralph Lapp argues, that there is a 'technological imperative - when technology beckons, men are helpless'.[2] Thus, according to the UN *Comprehensive Study on Nuclear Weapons* :

> It is widely believed . . . that new weapon systems emerge not because of any military or security considerations but because *technology by its own impetus* often takes the lead over policy, creating weapons for which needs have to be invented and deployment theories have to be readjusted.[3]

2. Ralph E. Lapp, Arms Beyond Doubt: The Tyranny of Weapons Technology (New York: Cowles Book Co., 1970), 178.

3. United Nations, Report of the Secretary General, Comprehensive Study on Nuclear Weapons (United Nations, New York, 1981), para 67, cited in Marek Thee, Military Technology, Military Strategy and the Arms Race (London: Croom Helm, 1986), 47; emphasis added.

In its most extreme form such technological determinism portrays the development of technology as though it were simply the inevitable application of science which itself can be seen unproblematically as the 'real world' revealed. Scientific discoveries are seen as being applied in technology which then has 'effects'. Such an interpretation of technical change seems, however, to rest on two dubious premises - that the content of scientific knowledge is simply and 'naturally' determined by the physical world, and that the science-technology relationship is one-way and causal.

In practical terms this would seem to imply that technical application would always trail scientific understanding. But thoughout the FBM programme there are many examples, in major areas of technology, where this has not been the case. The widely perceived advances in technology - in, for example, inertial guidance, re-entry vehicle design, and solid propellants - have come about, not because of advances in scientific understanding, but rather in spite of the lack of them. In each of these fields advances have relied on 'trial-and-error', 'rules-of-thumb', and all the other processes (including some input from 'science') that go to make up the eclectic craft of technologists.

Indeed technological change in these areas has itself been a major factor in furthering scientific understanding, rather than the reverse. However, while a major concern of scientists is theoretical - to produce a way of understanding the world that 'works', that of technologists is practical - to make an artefact that 'works'. What it means to work is, of course, in both cases decided by socially-mediated criteria. But it is certainly not the case that 'working' science is either sufficient or necessary to produce 'working' technology. The craft or 'black art' required to produce inertial components, solid propellants and re-entry vehicle nose-tips cannot be adequately documented in algorithms and formulae. Wise programme managers appreciate this - that the tacit knowledge involved in many technical skills is a key factor in success and that people transfer such skill much more fully than even the most exhaustive documentation.

Technology cannot, then, be seen as simply applied science, following a 'natural' pathway determined by the 'discovery' of the real world. Even if the production of scientific knowledge were itself such an unproblematic process, it still could not be considered the sole, or probably even the most important, factor in technological change. The creation of technology and of scientific knowledge are related processes, but the relationship is by no means one-way.[4]

But, setting aside the relationship with science, could technological developments still be seen as the inevitable consequences of 'manipulating' the physical world? Even if technical developments do not directly arise out of the 'laws of physics' does a physical reality still determine the pathways taken by technology? Can technological development be explained in terms of 'natural trajectories'? For example, is the progression from Polaris to Trident II the inevitable result of technological change in which advances in technology provided better accuracy, which in turn led to a change in targeting strategy? In 1973 it was predicted that: 'Just as MIRV was inevitable from the point of view of being a natural accumulating of technical knowledge, hard-target MIRVs also will be irresistible, policy statements to the contrary notwithstanding'.[5] A prediction which has been proven correct, even in the traditionally 'assured destruction' orientated FBM system. But can 'technology' be seen as the cause?

Based on the present study it cannot. The history of FBM technology cannot be seen as an inexorable, out-of-control, determinate and determining, path of technological development. The physical world does not, for example, naturally facilitate the development of high quality inertial components. On the contrary, this is achieved only with great difficulty and at great expense. Where high accuracy was not seen as required, such as in civilian and most military aircraft applications, inertial technology has followed a different course -emphasizing lowering

4. B. Barnes, 'The Science-Technology Relationship: A Model and a Query', Social Studies of Science, Vol. 12 (1982), 166-73.
5. R. L. Tammen, MIRV and the Arms Race: An Interpretation of Defense Strategy (New York: Praeger, 1973), 126.

life time costs, developing 'sweet' technologies, but of a different kind to those found in missile guidance.[6]

Trident II's expected high counterforce capability is, in any case, in no sense the unintended result of technical change. Such a capability (relative to the target hardness of the time) has always interested some people. As is clear from the Pentagon's Weapon System Evaluation Group Report of 1957, counterforce capability was even then considered an important criterion by which to judge the performance of ballistic missiles.[7]

But, whilst counterforce was enthusiastically taken up by the Air Force, the pressure for a counterforce FBM remained localized until the 1970s. Initially, at least, 'technical' differences based on the physical world made counterforce appear more difficult for FBM's (at least when comparing a mobile FBM to a fixed-silo ICBM), but this 'physical' distinction was a 'social' one too, as differentiation was deliberately chosen within the Navy. SPO's interest in projecting the image of the FBM as a counter-city deterrent, and a declared national strategy of assured destruction, held at bay the strategists who desired hard target kill and the technologists who felt they could provide it.

Moreover, even project managers wholly infatuated with technology cannot do everything that seems technically possible, or even everything that seems technically 'sweet'. Typically, there are choices to be made between different technical pathways. Sometimes these detailed trade-offs have only minor significance for the overall characteristics of the system; sometimes these little details may turn out to be highly significant.

Thus there was a choice between staying with pure inertial guidance or moving to stellar-inertial. Here the wider political context played a role in augmenting technical doubts which delayed the introduction of stellar

6. See D. MacKenzie, W. Rüdig and G. Spinardi, 'Social Research on Technology and the Policy Agenda: An Example from the Strategic Arms Race', in Brian Elliott (ed.), Technology and Social Process (Edinburgh: Edinburgh University Press, 1988), 152-80.

7. Though it seems likely that this report was written by people concerned to play down the utility of ballistic missiles for counterforce missions.

inertial guidance till Trident I. In another case technological developments (especially in computer capabilities) could have led to a switch from a stable platform guidance system to a strapdown one - but did not. Similarly there were choices over the type of accelerometer used - the larger, more expensive, and more accurate PIGAs (Pendulous Integrating Gyro Accelerometers) or the more economical PIPAs (Pulsed Integrating Pendulous Accelerometers) - and the size and numbers of warheads carried.

The technical choices made in the Special Projects Office involved explicit trade-offs between performance and cost, as well as debate about what were the correct goals and what were the best ways to achieve them. For example, the choice of warhead size and configuration in the Poseidon missile was heavily influenced by both strategic and bureaucratic factors, and was directly related to system accuracy. Technical choices were thus influenced by the 'macropolitics' of US defence policy, by the organizational politics of the Navy and its relationship with the Air Force, also by the 'micropolitics' of the technical community. Whether a star-tracker was needed to supplement pure inertial guidance was doubted by dominant opinion at the Instrumentation Laboratory, where there was a strong commitment to the achievement of ultimate accuracy by refinement of unsupplemented inertial sensors. The stellar-inertial option was pushed by outside industry - the Kearfott Division of Singer.

So there are many instances of 'technical choices' made in the Fleet Ballistic Missile programme, and good evidence of a range of 'political' and institutional factors shaping these choices. But 'the social' does not simply operate at the level of preferences between pre-defined technical options. It also shapes the options that are available, and may on occasion actually eliminate the possibility of explicit choice.[8] The social can enter into the definition of what is possible.

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8. See T. J. Pinch and W. E. Bijker, 'The Social Construction of Facts and Artefacts: or How the Sociology of Science and the Sociology of Technology might Benefit Each Other', Social Studies of Science, Vol. 14 (1984), 399-441; S. Russell, 'The Social Construction of Artefacts: A Response to Pinch and Bijker', Social Studies of Science, Vol. 16 (1986), 331-46; T. Pinch and W. Bijker, 'Science, Relativism and the New Sociology of Technology', Social Studies of Science, Vol. 16 (1986), 347-60.

One example of this concerns homing reentry vehicles, a possibility ruled out in the late 1970s in part because adequate testing of the technology was deemed infeasible. To date US strategic ballistic missiles have been tested over water, usually impacting either in broad ocean areas or in the Marshall Islands, where indigenous Islanders have so far been unable to bring significant legal or political pressure to bear. So the political power of citizens of the mainland US by comparison with Pacific Islanders is a cause of adequately-tested homing reentry vehicles, which would require overland testing, being seen as infeasible.

'Soft' Determinism - Enablement and Constraint

Clearly, then, a 'hard' form of technological determinism -in which artefacts are seen as the result of 'applied science' or as following inevitably from their predecessors due to natural trajectories, or in which the nature of programmes is determined by available component technologies - cannot be sustained. At the same time 'technology' is not simply a dependent variable either. 'Technology' can sometimes be important as an enabling capability or a limiting constraint.

Technology as enablement amounts to a very 'soft' form of determinism, if it can be so termed at all. Basic technologies and sciences may feed back into specific developments, both allowing and perhaps stimulating technological advances. Thus work in computing, inertial instruments, geophysics and geodesy has enabled missile accuracy to be greatly improved. Schroeer's claim that computing capabilities have driven missile accuracy might seem a case in point.[9] However, increased computer capabilities could just as reasonably have led to strap-down guidance systems (had cost and ease of maintenance been more important than accuracy) rather than better performance of stable platform designs. Enabling technologies merely provide possibilities, they do not determine the course actually followed. Rather than Schroeer's 'technological imperative' a more suitable term might be Shapley's 'technology creep'.[10]

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9. Dietrich Schroeer, 'Quantifying Technological Imperatives in the Arms Race', in D. Carlton and C. Schaerf, Reassessing Arms Control (Macmillan, 1985), 60-71.
10. D. Shapley, 'Technology Creep and the Arms Race: ICBM Problem a Sleeper' Science, Vol. 201 (September 22, 1978), 1102-05.

Furthermore, advances in some basic technologies and sciences, such as inertial instruments, geophysics and geodesy, have to a large extent been driven by military requirements for more accurate guidance systems. Even where technological and scientific advances on a broader front feed back into specific military technology, as computers seem to have in this case, it is important not to forget how those advances come about, and are themselves socially shaped. Widely applicable developments need not be unstoppable if no-one has a particular interest in pursuing them because, paradoxically, everyone may leave it to some one else to pick up the costs. Thus the development of the US Global Positioning System of navigation satellites may have been slowed because all of the armed forces expected to get access to it, but none especially wanted to pay for it.

The development of specific enabling technologies may also be slowed, or stopped, if social conditions are not right. The very high quality inertial components used in strategic missile guidance have few other perceived uses. Recently even advocates of counterforce accuracy in strategic missiles have begun to doubt whether further improvements in inertial sensors would provide much gain in overall system accuracy. Funds for this form of development of the enabling technology have thus been proving hard to find, and the Draper Laboratory's efforts to develop 'fourth generation' inertial instruments significantly more accurate than the 'third generation' instruments of MX and D5 have not found sources of external support.[11] The 'technical trajectory' of further refining Draper floated instruments may no longer be socially viable.

But 'technology creep' not only increases the availability of tempting new technologies, it can also lead to the non-availability of satisfactory old ones. In some areas of the FBM programme, such as the launcher subsystem, the 'latest' technology has often been only grudgingly introduced. For example, the launch system initiator which had remained the same design up until Trident II:

> One other new element that's just been introduced for Trident II, we're just introducing it now, we haven't finished developing it, is what we call EBWTBI, electronic bridge wire through bulk head initiator, its a new device to

[11]. See MacKenzie, Rüdig and Spinardi, 168-69.

> initiate the burn in the gas generator, the device that we have used up until now is an initiator that was designed for use in Polaris missiles and did initiate some of the motors in Polaris, we used it for our purposes but it is now a very ancient design, the manufacturer is no longer making it and it did not seem feasible to go back and start that up again . . . often times we are forced into changes not because in and of itself our program dictates the changes, but rather the market place dictates the changes, something is no longer available because no-one else uses it. For whatever reason other usages have disappeared, they've gone to other technologies, so that although we may not need ourselves to go to another technology, if everyone else has and no longer do we have a suitable source of supply, then we may have to look at this new technology too in order to widen our source of supply. That's what's happened in this initiator, the old initiator is simply out-of-date as far as the rest of the world is concerned so that although I'd be happy to continue to use it, I can't.[12]

Thus to some extent the 'market' not only enables, but also constrains by determining the technology that is available, at least at a reasonable price.[13] But 'technology' may also prove a limiting constraint in other ways. Most obviously, in a complex weapons system such as the Navy ballistic missiles, design interfaces need to be fixed and adhered to. From that point on subsystem technology is constrained by the need to 'fit'. Design decisions may thus have an enduring physical legacy restricting future technology. For example, the physical size of submarine missile mount tubes (which is of course itself limited by the size of the submarine) limits the size of missile possible. All the US Polaris class submarines built in the 1960s had the same size mount tubes. Once some initial extra space had been accounted for, when Poseidon missiles were installed, there remained no space for further expansion. When the Trident I missiles were designed to fit the same submarines, but also to provide about a third more range, this proved a severe constraint, which required much ingenuity to overcome.[14] Moreover, had it not been decided to enhance crew comfort by enlarging the Polaris submarine

12. Interview.

13. Obviously it is usually possible to continue producing a technology that is considered obsolescent if one is prepared to pay enough, as indeed the UK did when its Polaris A3 required the renovation of its solid-fuel motors.

14. See L. Smith, R. H. Wertheim and R. A. Duffy, 'Innovative Engineering in the Trident Missile Development', Bridge (National Academy of Engineering), Vol. 10, No. 2 (Summer 1980), 10-19.

design after the first ten, then sufficient buoyancy would not have been available to allow the back-fitting of the Poseidon and Trident I missiles.

But such technical constraints may also be understood as social or political. In the debate over Trident, the 'technical' argument for a larger missile was deployed by those in the Navy who favoured building a new, much larger class of submarine. In particular, Admiral Rickover, the 'father' of nuclear propulsion, argued strongly in favour of a large submarine because it would require a new nuclear propulsion system. SPO, on the other hand, were sceptical of the idea of putting more, larger missiles into a bigger submarine. A particular concern was that this meant fewer targets for Soviet anti-submarine warfare, but the question of size was also connected to disputes over the division of responsibility between Special Projects and Admiral Rickover. SPO would have preferred to live with the 'technical constraint' of missile tube size - at least for a few more years - if this had at the same time constrained Rickover's influence on the FBM programme by weakening the 'technical' argument for a new propulsion reactor.

Technologists-out-of-control?

Technological development thus has the potential to follow a number of different courses, rather than one single predetermined pathway. It provides capabilities and sets some constraints, but is profoundly shaped by the social world. Most immediately, of course, it is shaped by the technologists who develop it, and who have a considerable interest in promoting it. Personal preferences and institutional interests mean that many technologists like to push technologies as far as they can in particular directions. With regard to improving the accuracy of Poseidon by using a star-sensor, one Special Projects engineer's attitude was: "I could provide it, so I should provide it".[15] But as we have seen this stellar-inertial system was not incorporated into Poseidon. Indeed Special Projects' Director, Vice Admiral Levering Smith built up a considerable reputation for not choosing technologies simply because they were 'sweet':

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15. Interview.

'Smith's strength was to know that if you could do it, it didn't necessarily mean you ought to'.[16]

Such attitudes are important and Admiral Smith's personal style shaped the character of FBM technology. But just how important depends on various social relations. In the late 1950s Raborn and Smith had considerable freedom and resources with which to shape the FBM programme as they desired. But by the 1980s SSPO's leadership had to cope with a much more interfering 'outside world' as well with a history of organizational relations that limits their behaviour.

But what evidence is there that technologists are, as Lord Zuckerman amongst others has suggested, manipulating the strategic and political environment so as to create a market for their favoured technology?[17] Certainly in the Navy ballistic missile programme crucial technological advances were developed externally and before any 'requirement' had been formulated for them. The initial breakthrough of small high-yield warheads, the introduction of stellar inertial guidance, and the gravity gradiometer installed in Trident submarines are all such cases. However, none found automatic acceptance without careful scrutiny as to how they would affect the goals and interests of the programme. Only if technologists could transform these goals and interests so that they then fitted their preferred technology would the technologists-out-of-control thesis have significance.

Undoubtedly they have tried. For example, Charles Stark Draper clearly perceived that he was trying to engineer people's attitudes as well as guidance systems:

> ...I had always been interested in people that I had to deal with and their mental attitudes and why they had these mental attitudes and choosing something that the people in charge had to have.[18]

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16. Interview.

17. Lord Zuckerman, 'Science Advisers and Scientific Advisers', Proceedings of the American Philosophical Society, Vol. 124 (1980), 124-55.

18. Interview.

Not 'wanted', but 'had to have'. There are indeed several instances in the history of the Navy ballistic missile programme where technologists sought to persuade 'the people in charge' that they had to have something they had not originally wanted. But these are generally cases where the technologists convinced 'the people in charge' that their existing goals could be better fulfilled by a new device, not where they changed these goals to further a preferred technology.

One candidate case of this is Draper's attempt to persuade the Navy to rival ICBM accuracy.[19] But this was a failed attempt. The only case which might be classed as a success was the attempt to 'sell' stellar-inertial guidance by reorientating the FBM from assured destruction to counterforce. But even this is not clearcut. The attempt failed in the case of Poseidon. It suceeded with Trident C4, but only as part of a 'repackaging' that emphasized stellar-inertial guidance's compatibility with assured destruction.

So the goals of the programme managers have by no means been putty in the hands of ambitious technological enthusiasts. Indeed FBM programme managers have been actively aware of the threat that over-zealous technologists can pose to their programme's success. A former Director of SSPO recalled that:

> . . . there's always going to be a technologist somewhere who doesn't feel comfortable until you've applied all the technology you could possibly apply. And the problem of the program manager, of course, is to try to take all this and balance it and make a prudent choice of what he thinks can be done and can be done within a program cost and schedule that he can predict and is good enough to meet the need. . . . You simply can't afford to put all the technologies into a system that the technologists can promise.[20]

19. See page 80.
20. Interview.

Weapons Succession, the Military-Industrial Complex and 'Internal' Arms Races

Neither 'technology' itself nor the technologists that develop it would then seem to have determining roles in technological developments. That it not to say, of course, that previous technology and the preferences of technologists are not very important. They are - in both setting historical constraints on technology which builds on that of the past (both literally, as in the physical constraints of, say, a submarine missile mount tube, and culturally, in the ideas and paradigms that provide the intellectual basis that technologists draw on) and in shaping the interests of the technical community.[21]

It is possible, however, to have a wider analysis which goes beyond such a narrow definition of the interests of the technical community, but which still argues that a kind of 'technological imperative' exists. Thus, for example, Mary Kaldor has argued that the nature of military R&D organizations, particularly in the USA, determines the type of technology developed.[22] In the USA this technology is typically 'baroque' - produced through the continuing 'improvement' of several performance parameters, such as speed, accuracy, range, but in which the '"improvements" become less and less relevant to modern warfare, while cost and complexity become military handicaps'.[23] Baroque technology, Kaldor argues, is produced by sovereign R&D establishments which are nevertheless heavily dependent on government contracts - that is, defence orientated corporations such as Lockheed. Preoccupied with maintaining full capacity employment they emphasize the continual 'improvement' of weapons along conservative, already-established lines - 'normal' rather than radical technology:

> These large firms emphasize risk minimization and thus tend not to push new ideas or applications. Research is more likely to be done on increasing the performance of a device, rather than developing some totally new device.

21. Which in the case of the nuclear weapons laboratories has lead to very active lobbying against nuclear test ban treaties.
22. Mary Kaldor, 'Military R&D: Cause or Consequence of the Arms Race?', International Social Science Journal, Vol. 35, No. 1 (1983), 25-45.
23. Mary Kaldor, The Baroque Arsenal (London: Andre Deutsch, 1982), 5.

> This 'evolutionary' R&D tends to match the forms and objectives of the firms and the DoD and even to address the questions these firms are willing to ask. (More far-reaching questions would pose a threat to existing organizations - an airplane manufacturer would not want the usefulness of airplanes questioned, nor would a military pilot.)[24]

The succession of FBM generations would at first sight seem a classic instance of baroque improvement. Once established as the missile contractor for Polaris, Lockheed Missiles & Space Co. has received non-competitive contracts for suceeding generations of ever more elaborate missiles. However, a more detailed look at the FBM history suggests that the 'baroque' explanation of weapons succession has only general utility.

Organizational interests will generally tend to exclude radical new technologies (especially if 'not-invented-here'). Everyone, whether private corporation, government-sponsored non-profit organization (such as the Draper Laboratory and Johns Hopkin's Applied Physics Laboratory), or military (such as SSPO), prefers continuity. Radical technological developments can threaten such continuity, whereas 'evolutionary' technical change reinforces it. But that alone does not explain *which* parameters should be chosen for incremental 'improvement', why in the FBM programme first range should be considered critical (from Polaris A1 to A2 to A3), then ABM penetration (in Poseidon), then range again (in Trident I), and finally accuracy (in Trident II). Nor does it explain how radical innovations were introduced, such as MRV in Polaris A3, MIRV in Poseidon, stellar-inertial guidance in Trident I or the gravity gradiometer for Trident II. These are not just more of the same, incremental changes. Moreover, thoughout the FBM development, reliability has remained a central attribute which increasing cost and complexity do not appear to have compromized. In some areas of the system, such as the launcher subsystem, much of the technology remained unchanged whilst the Polaris submarines were updated to carry first the Poseidon and then the Trident I missiles. Often indeed such technology would only be replaced by a new one because the original was too 'obsolescent' for replacements to be obtained at a reasonable price.[25]

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24. Jacques S. Gansler, The Defense Industry (Cambridge, Mass.: MIT Press, 1980), 101.
25. Interview.

Nor does the Trident submarine development - which in many ways appears to be a classic example of 'baroque' technology - exactly fit an explanation based on corporate determination to maintain (and if possible expand) its operations. Although Electric Boat's corporate interests played a role, it was mainly driven by the technical preferences of Admiral Rickover of the Navy's Nuclear Propulsion Directorate. But as a state-financed, government-dependent organization this should, in Kaldor's classification be a source of 'conservative' technical change.[26]

What this suggests is that although Kaldor's distinctions between the differing styles of different R&D organizations may be useful, they cannot alone explain the nature of technical change. 'Baroque' technology is a symptom of recent US weapons developments because continuity is in the interest of most of the organizations involved. But it is not just the nature of the organizations that matters, but also their interactions with others. The Draper Laboratory has been continually improving its paradigmatic floated gyroscope since the 1940s[27] - its most baroque manifestation is the MX gyroscope. But under pressure from the Navy they did eventually agree to incorporate a steller sensor into their guidance systems, knowing, of course, that this would tend to undercut future arguments for further gyroscope improvement. An explanation of technical change in weaponry must take into account such organizational interactions.

A further variant of the role of organizational interactions in stimulating technical change suggests that it is the product of an 'internal' arms race. For example, Ernest Yanarella suggests that a 'technological imperative' was institutionalized in the USA at the end of the 1950s and the beginning of the 1960s.[28] Organizations set up to help the Office of the Secretary of Defense manage advances in military technology - most notably the Office of the Director of Defense Research and Engineering (DDR&E) and the Advanced Projects Research Agency (ARPA) - provided

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26. Kaldor, 'Military R&D', 42.

27. See Donald MacKenzie, 'Missile Accuracy: A Case Study in the Social Processes of Technological Change', in Wiebe E. Bijker, Thomas P. Hughes and Trevor Pinch, The Social Construction of Technological Systems, (Cambridge, Mass.: MIT Press, 1987), 208-09.

28. Ernest J. Yanarella, 'The "Technological Imperative" and the Strategic Arms Race', Peace & Change, Vol. III, No. 1 (Spring 1975), 3-16.

a focus for the assessment and encouragement of change in miltary technology, and, Yanarella argues, 'institutionalized new sources of dynamism into defense planning at the pinnacle of the administration'.[29] This was followed by the further centralization of weapons technology decision-making - at an unprecedented level of technical detail - into OSD during McNamara's tenure as Secretary of Defense, and by doctrinal reassessments of nuclear strategy:[30]

> Centralized in the executive agency of the Defense Department and guided by the most advanced techniques of administration and analysis, military R&D in strategic weaponry was institutionalized and pursued during the McNamara years in an organizational framework characterized by the mutual interaction of military R&D in offensive technology with military R&D in defensive technology. . . the "technological imperative" took on nearly all the features of an "internal arms race" pitting, within the same agencies, American scientists and technicians in offensive R&D against their counterparts in defensive R&D.[31]

A classic example of the 'mirror imaging' consequent of such an 'internal arms race' can be found in the FBM programme. The configuration of the Polaris A3 payload was designed to defeat the then proposed US ABM system, the Nike Zeus, and turned out to have little capability against the Soviet Galosh:

> The penetration aid designs done originally for the Polaris A2 and also for the Polaris A3 were built around the notion of an antiballistic missile which looked very like America's Nike Zeus which was our design for an ABM system. Since we didn't know what the Soviets were doing we assumed that they were being smart people, were doing exactly what we were doing.[32]

That such 'mirror imaging' is an important factor is not in doubt, but to what extent does it constitute a technological imperative, and to what extent did the period in question mark a distinctive change? Was it really

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29. Ibid, 6.

30. Notably, the (admittedly somewhat arbitrary) quantification of 'assured destruction' and of the counterforce and 'damage-limiting' funcions of strategic nuclear forces. See Alain C. Enthoven and K. Wayne Smith. How Much Is Enough? Shaping the Defense Program, 1961-1969 (New York: Harper & Row, 1971), esp. Chapter 5.

31. Yanarella, 9.

32. Interview.

the case that 'this technological planning process . . . increasingly adopted the features of a closed system where interest in the character of the Soviet threat, Soviet perceptions of specific weapons programs, and other "external" data were of secondary importance to "internal" requirements of the system'?[33]

In fact, of course, 'mirror imaging' was not a new phenomenon in military planning - as can be seen by looking back at the previous decade. McNamara's OSD may have brought a more analytical emphasis to assessing and justifying weapons requirements - and made explicit, for example, how much 'assured destruction' was enough - but the basic processes remained the same. Only previously each service would produce its own distinctive analysis of the threat and of their requirements to counter it. Indeed in a 1961 briefing to the Secretary of Defense the Navy was still justifying a figure of 45 FBM submarines without mentioning the nuclear forces of the other services:

> The briefing began with a list of targets to be destroyed, a calculation as to how many missiles should be programmed per target, how many were needed on station, how many were needed in the total force to maintain that number on station, and thus why a force of 45 Polaris submarines was required. In the entire briefing, there was not one reference to the existence of the Air Force or its weapons systems, despite the fact that most of our nuclear firepower was then in Air Force bombers.[34]

What each service 'required' depended primarily on their dominant technological and organizational traditions, and on what looked likely to enhance their portion of the defence budget. Radically new technologies, such as ballistic missiles, were thus not 'required' as much as the more traditional weapons, such as bombers and aircraft carriers. Moreover, throughout the 1950s the size and nature of forces remained largely up to the services within the constraint of their budget allocation. In so much as weapons acquisition required justification each service constructed its own rationales based on its own interpretation of intelligence data. Thus during the 1950s the Air Force acquired huge numbers of nuclear bombs -

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33. Yanarella, 9.
34. Enthoven and Smith, 171.

and a large bomber force to carry them - by 'finding' ever more Soviet targets to attack.[35]

Lacking any coherent, consensual, systematic way of analysing the utility of nuclear forces, the main focus of the 'internal arms race' during the 1950s was inter-service rivalry. Technological developments were no less sought than in the 1960s, they were simply less channeled towards specific, explicitly defined *national* missions. Indeed weapon systems development proliferated during the 1950s so that by the end of the decade the Air Force alone was engaged in programmes for three ICBMs (Titan, Atlas and Minuteman), an IRBM (Thor), an air-launched ballistic missile (Skybolt), several cruise missiles (Snark, Matador, Mace, and Hound Dog), three conventional bombers (B52, B58 and B70) and a nuclear-powered bomber. This last programme, known as the ANP (for aircraft nuclear propulsion), involved a curious example of 'rhetorical' mirror-imaging. Under development since just after World War II the ANP programme came under particularly critical examination in the late 1950s as its feasibility and utility were questioned.[36] Coincidentally proponents of the ANP suddenly revealed that the Soviet Union was flight testing a similar aircraft and the journal *Aviation Week* even published sketches of it.[37] The ANP was eventually cancelled by the Kennedy Administration, and with it, its Soviet mirror-image vanished too.

It was as a response to this uncoordinated technological proliferation of the 1950s that the offices of DDR&E and ARPA were formed, and McNamara's more centralized managerial methods, such as the Planning-Programming-Budgeting System, introduced.[38] By explicitly defining the purposes of nuclear forces - using criteria such as 'assured destruction' - these changes did certainly channel technological change in particular directions. At the same time, however, they also provided OSD with the 'tools' that allowed many other programmes, including Skybolt, Snark, the B-70 and ANP, to be cancelled. The changes institutionalized in

35. See D. A. Rosenberg, 'The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960', International Security, (1983), 50.
36. See Herbert York, Race to Oblivion: A Participants View of the Arms Race (New York: Simon and Schuster, 1970), chapter 4.
37. Aviation Week (December 1, 1958), 28.
38. See Enthoven and Smith.

OSD were as much a check on technological developments as a part of their stimulus.

In any case the 'technological imperative' of the 'internal arms race' is neither completely impervious to 'external' events (no matter how much intelligence of Soviet behaviour is mediated by inevitable judgemental interpretation), nor a strong determinant of the nature of technical change. An 'internal arms race' between offence and defence would be expected to stimulate the development of ABM technologies to destroy incoming missiles, on the one hand, and penetration aids to maintain 'assured destruction', on the other. This might appear to have been the case in Poseidon where large numbers of small warheads were chosen to enhance penetration against the possibility of widespread deployment of Galosh-type ABMs (which, of course, drew at least partly on knowledge of what the Soviets were doing). It would not explain, however, why at the same time the Air Force Minuteman MIRV placed so much more emphasis on the ability to attack hard targets rather than penetration, or why subsequent developments in Trident I and II would increasingly emphasise counterforce capability with no significant penetration aids produced.

In fact, the notion of an 'internal arms race', important though it might be as a stimulus for R&D, cannot account for the importance of the growing mutual disillusionment with ABM defences during the 1960s that eventually led the USA and USSR to sign the ABM Treaty in 1972, nor for the interest in counterforce (and its differing attraction for the Air Force and the Navy). The closed system of an 'internal arms race' fails to account for the importance of these broader influences in shaping technological change.

Similar problems also occur when attempting to explain weapons procurement as the result of a yet still more encompassing domestic influence - that which has come to be known as the Military-Industrial Complex.[39] Briefly characterized, this viewpoint explains high levels of military spending in the USA as the result of the 'vested interests' of the

[39]. For a useful collection of essays, see Steven Rosen (ed.), Testing the Theory of the Military-Industrial Complex (Lexington, Mass.: Lexington Books, 1973).

military, defence corporations, governmental and legislative elites, defence-related scientists and technologists, and other pressure groups (such as right-wing 'think-tanks' and veterans associations).

Weapons procurement and defence R&D provide the links by which corporate, scientific-technical and military interests mutually benefit. As a corollary of this it would not be surprising to find that these groups are mutually supportive, nor that they would be actively encouraged by regional legislators who would appear likely to gain political benefit from any creation (or retention) of jobs in their locality. Given the levels of funding which go to defence R&D and procurement in the USA this 'network' of interests is likely to form a powerful force pushing technology.[40] But to what extent can it seen as a determinant of weapons technology?

For example, can the apparent phenomenon of the 'follow-on imperative'[41] - whereby defence corporations are kept in business through the awarding of contracts for successor weapons systems - be explained as the result of the machinations of the Military-Industrial Complex? The history of FBM technology, with each suceeding generation non-competitively awarded to Lockheed, would appear to be a classic case of such a 'follow-on imperative'. However, what has been called the 'weapons succession process' cannot be explained simply in terms of corporate profit (or survival).

Such economic arguments for weapons succession fail to explain technical change. If the sole factor were maintaining corporate profitability or survival then this might be achieved by continued production of the same technology, with technical innovation restricted to improving the *process* not the *product*. But, in fact, the opposite is typically the case - technical innovation focuses on product, not process. As already noted, the weapons succession process is characterized by its emphasis on product innovation, producing 'gold-plated', 'baroque' technology.

40. A prime example, perhaps, of what Langdon Winner sees as the corporate basis of technological momentum. See Autonomous Technology: Technics-out-of-control as a Theme in Political Thought (Cambridge, Mass.: MIT Press, 1977).
41. See James R. Kurth, 'American Production Lines and American Defense Spending', in Rosen (ed.), 135-56.

Indeed it would seem a general failing of Military-Industrial Complex theories that whilst they may account for the scale of resources allocated to military technology, they provide little explanation for the nature of technical change. A variety of 'vested interests' may share a common interest in, and gain mutual benefit from, high levels of spending on the development and production of weapons technology, but they are unlikely to exhibit such unanimity on exactly which projects should benefit. The Military-Industrial Complex may be an identifiable and powerful alliance of interests, but it is not without internal divisions.

An instance in the FBM programme which might seem to typify the workings of the Military-Industrial Complex was the introduction of stellar-inertial guidance. Here two Kearfott Presidents (Marvin Stern and John Brett) passed through the 'revolving door' between Government and the defence industry- one joining Kearfott after working in the Defense Department, the other temporarily leaving Kearfott to work in OSD. Both were to be influential advocates of stellar-inertial guidance, playing important roles in getting it adopted initially for the Poseidon Mk4 guidance system, and then actually deployed in Trident I. In this they were clearly heterogeneous engineers, attempting to build a network of interests to support their favoured technology. In Poseidon their success was shortlived, when it seems a tactical mistake was made in attempting to promote the acceleration of the introduction of stellar-inertial guidance by reference to overt counterforce capability. The lesson was learned, and for Trident I John Brett was influential in shaping the system's goals in ways which would favour the attributes of stellar-inertial whilst staying within the context of 'assured destruction'.

As already noted, the managers of weapons programmes are confronted with many such 'salesmen', pushing their various technologies. But only a few are successful. Given these competing interests (between the Air Force and the Navy, say, or the Draper Laboratory and Kearfott) what actually decides the nature of technology? Are the final arbitrators the politicians, in the Administration and Congress, who ultimately are 'in charge'?

Politics-in-Command?

Explanations which assert the primacy of political control over weapons procurement can be separated into two kinds. Firstly, there is the importance of the electoral process as a determinant of weapons technology or the 'democratic explanation'.[42] For example, a persuasive case has been made for the broad effect of Democrat politicians boosting military spending because of over-reaction to 'hawkish' criticism.[43] Thus President Kennedy, having 'milked' full electoral value out of the 'missile gap', sought a quick acceleration of the Polaris programme. Similarly, (but with a Republican), on taking office Nixon instructed Defense Secretary Laird to bolster the defence posture in a visible, but cheap way - one result being the acceleration of the stellar-inertial option for Poseidon. But though persuasive, this version of 'politics-in-command' is not analytically far-reaching: it can indeed be seen as simply an adjunct to the 'bureaucratic politics' position reviewed below. Of much greater analytical importance is the second version of politics-in-command, in which weapons procurement is seen simply as the result of rational decision-making by unitary actors in response to military and political requirements determined by the 'threat'.

Thus, in his famous San Francisco speech announcing his (clearly reluctant) intention to go ahead with a 'thin' ABM system purportedly to defend against Chinese nuclear weapons, Secretary of Defense McNamara enunciated an 'action-reaction' theory of the arms race:

> What is essential to understand here is that the Soviet Union and United States mutually influence one another's strategic plans. Whatever their intentions, whatever be our intentions, actions - or even realistically potential actions - on either side relating to the build-up of nuclear forces, be they either offensive or defensive weapons, necessarily trigger reactions on the other side. It is precisely this action-reaction phenomenon that fuels an arms race.[44]

42. Ibid, 136.
43. Alan Wolfe, The Rise and Fall of the Soviet Threat: Domestic Sources of the Cold War Consensus (Boston: South End Press, 1984).
44. Quoted in Thee, 112.

As such the 'action-reaction phenomenon' focuses on 'external' determinants of weapons technology. The technology was a response to what the other side - the Soviets (or even the Chinese) - was doing. Thus, for example, Poseidon's MIRV design is said to have been a response to the appearance of the Gal osh ABM. The problem with the action-reaction phenomenon in practice is that the reaction has often been premature, excessive or even completely inappropriate.

The reasons for this would seem, at root, to be 'internal' in two ways. Firstly, what the 'external' world consists of is not unambiguous, and what it will look in, say, five years must be speculative. No matter how 'technically' proficient intelligence collecting methods are, the data collected still need to be analysed and interpreted. Secondly, even when a consensus is reached about what the 'external' world looks like, it still remains to be decided what is to be done about it. So although 'external' actions are an important input they would by no means seem to determine what the output - the reactions - will be.

And even once such 'decisions' as to the most appropriate reaction were reached there remains the question of whether technical change is something that can simply be directed by political elites. Just how powerful are the President, the Secretary of Defense and Congress (to name the major actors)? On the face of it, there is much evidence which suggests that these political elites can be very important in 'deciding' important attributes of weapons technology.

For example, pressure from the Office of the Secretary of Defense was important in the improvement of missile accuracy. Most obviously Secretary Schlesinger's 1974 review of nuclear strategy towards more flexible Limited Nuclear Options and his pressure for an Improved Accuracy Program were especially significant. However, it is clear from this example that this was not a question of command in the archetypal military sense. 'Pressure' is indeed the appropriate term here as political leaders sought to obtain greater accuracy from the Navy missiles. For two reasons it was simply not possible to command the technology into being.

First of all there is a general limitation to command. Technological change simply does not possess either the transparency or the predictability that would be required for straightforward command to be possible. Dominant social groups, whether these be political, business or military elites, are typically in no position to shape technological change in the conscious, literal sense that we can imagine an artist moulding clay. If they choose between given technological options, then those who present those options to them have an opportunity both to set the agenda (deciding which options to present) and to influence the decision in the way they portray the advantages and disadvantages of different options. If they seek to create a new technology, then they may well need advice as to feasibility - for what they desire may be physically impossible, or hopelessly expensive, or whatever. Sometimes, indeed, an elite group may simply be unsure precisely what its interests and goals are, at least in the area in question.

Secondly, and more specifically, politics-in-command fails to provide an adequate explanation in the US political context. Here a combination of democratic political system and complex and overlapping jurisdictions creates a situation where political leaders such as a President or Secretary of Defense have to engage in a process more akin to bargaining than to giving orders.

Clearly, however, there are certain positions in the formal political system which can be very important influences on weapons technology. Depending on their personal interests, the President, Secretary of Defense, Secretary of State, National Security Advisor, head of the CIA, and Chairs of key Congressional committess can all wield considerable influence. Ultimately a Presidential decision (such as that by President Reagan to advocate strategic defenses) can be a powerful event. But no matter how persuasive an advocate the President is, and regardless of his formal position as commander-in-chief, his power can be formally limited by Congress, informally limited by obdurate bureaucracies, and eventually limited by a maximum tenure of eight years.

Typically the Administration and Congress have played a mainly passive role in managing the development of weaponry. Within funding limits and a general post-war consensus on 'national security' the fine detail of weapons characteristics, numbers and operational strategy have been largely left to the services. An exception, of course, was during McNamara's term as Secretary of Defense when many such issues came under close scrutiny by OSD. Since then, though not to its former extent, defence 'decision-making' has become decentralized again. Most of the time Congress and the Secretary of Defense will 'rubber stamp' the services recommendations - at least on issues considered to be 'technical' - so long as they accord with the overall defence policy.

This policy is itself (to a large extent) institutionally ingrained. It is embedded both in doctrinal and operational documents such as the Nuclear Weapons Employment Plan (NUWEP) and SIOP, and also in unwritten organizational 'culture'. That is not to say that it is static, of course, that it is not contested amongst the various actors involved, or even that it is internally consistant. But a new administration certainly does not start with a clean slate on which it can enscribe its preferences. President Carter's initial interest in changing US nuclear posture to dependence on only a few FBM submarines so as to provide 'assured destruction' retaliation provides an illustrative example.[45] Such a proposal, naïve as it no doubt appeared to insiders, was not inconsistant with the bulk of US declaratory statements about strategic deterrence made over the previous decade. These seemed to indicate that the potential for 'assured destruction' of a substantial part of Soviet urban-industrial areas (that is, cities) was the central plank of deterrence. So why not simply rely on a few of the apparently invulnerable Poseidon-carrying submarines? The answer was, of course, that 'assured destruction' was only the declaratory rationale for nuclear weapons, it was not the 'real' reason why most defence insiders supported a large, diversified 'triad' of nuclear weapons with varying capabilities, including some (such as high accuracy ICBMs) which made little sense simply for assured destruction.

As President Carter would come to learn, these reasons included not only rational (if somewhat esoteric) arguments about the political utility of counterforce and the symbolic need to match (or exceed) Soviet

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[45]. See T. Powers, 'Choosing a Strategy for World War III', The Atlantic Monthly (November 1982), 82-110.

capabilities, but also domestic politics such as interservice rivalry. Carter was persuaded to drop his radical proposals as he came to view the world through the eyes of the 'defence establishment', attuned not only to the Soviet 'threat', but also to the political need to pander to various domestic constituencies. Moreover, even his decision to cancel the B-1 bomber, which 'required' concessions elsewhere, would provide only a temporary hiatus for that programme.

Perhaps instead of focusing on the individual leaders it might instead be possible to explain weapons developments as the logical fulfilment of doctrinal requirements embodied in various national security docu ments, such as the SIOP and NUWEP? These, along with unwritten organizational traditions, might be the things that really guide weapons procurement and provide consistency from one administration to another. Is there evidence to suggest that it is in fact nuclear doctrine that determines the nature of weapons technology?

The answer would seem to be only up to a point, for two reasons. First of all, assuming there was a coherent doctrine formulated to guide weapons developments, this would only say what the technology was required to do. For example, high accuracy could arguably be obtained by improved inertial guidance, stellar-inertial guidance, mid-course Global Positioning System updates or terminal homing guidance. The second difficulty with the notion of 'doctrine-in-command' is that it by no means seems the case that the coherent formation of doctrine necessarily precedes and directs developments in weapons technology.

Since its first incarnation in 1960 the targeting warplan, the SIOP, has always been a capabilities plan, utilising whatever forces were available (although with increasingly more flexible options in recent versions). Following its revision in 1962 the SIOP remained unchanged for over a decade, during which time 'assured destruction' rose to prominence as the public face of deterrence. But although the 'damage limiting' counterforce mission was eventually disavowed, counterforce accuracy remained doctrinally justified by the need 'to follow a policy of limiting our retaliatory strikes to the enemy's military targets and not attacking his cities if he refrained from attacking ours'.[46] Along with the continuing emphasis on 'assured destruction' nuclear doctrine thus provided a generalized rationale for high accuracy ICBMs and lower accuracy (but less vulnerable) FBMs.

For much of the time the doctrine relating to nuclear weapons has consisted of disparate and conflicting elements. A public emphasis on 'assured destruction' has obscured a SIOP which contained a substantial counterforce element. Arms control policy seemed to say that it was important not to threaten the ability of either side to retaliate (as codified in the 1972 ABM Treaty), but procurement policy encouraged the development of hard target capable MIRVed ICBMs which did exactly that. It was only under Carter that nuclear doctrine came to favour counterforce, both explicitly and publicly. At last declaratory doctrine was to be brought into line with procurement policy, which, at least in the Air Force, had devoted considerable resources to the development of counterforce capability.

Even then nuclear doctrine does not so much appear to 'direct' technical change as to go hand in hand with it. Key figures in an administration will often be involved in questions of technical and doctrinal innovation at the same time. Thus Secretary of Defense James Schlesinger was involved in publicising the change in nuclear doctrine towards 'Limited Nuclear Options' (which was codified in the Nuclear Weapons Employment Plan or NUWEP in 1975 and introduced into a new version of the SIOP in January 1976) at the same time as he was pushing the Navy to provide more accuracy in its FBM system. But at least publicly he stressed that 'Limited Nuclear Options' did not depend on increased accuracy.[47] Similarly the thinking in the Carter administration that eventually lead to the promulgation of Presidential

46. Draft Memorandum for the President. Subject: Strategic Offensive and Defensive Forces (Revised January 15, 1968), 9.

47. Thus in testimony before the Subcommittee on Arms Control, International Law and Organizations of the Committee on Foreign Relations, United States Senate, March 4, 1974, reprinted in Robert J. Pranger and Roger P. Labrie (eds.), Nuclear Strategy and National Security: Points of View (Washington, DC: American Enterprise Institute for Public Policy Research, 1977), 104-17, at 105-06 Schlesinger says that: 'The change in targeting doctrine does not require new capabilities.... We are asking money in this budget for... some improvements in accuracy, but the change in targeting doctrine does not depend for its efficiency upon getting this money'.

Directive 59 involved people who were also pushing for accuracy in Trident II and MX. One such figure recalled how this new pronouncement about national strategy from the top served to crystallize changes that were already happening:

> With the PD-59 story having been pulled together that provided everybody with the theological framework around which you could rally and say we need it [counterforce] because of the following. It's national policy, it's no longer intuitive judgement and whimsy...[48]

Doctrine is thus related to weapons development, though declaratory doctrine may not tally with operational plans, and neither may be entirely compatible with procurement policy. Indeed these various aspects of doctrine are, like the weapons technology itself, the outcomes of many relationships between a variety of actors. They are inter-related, but none can be seen as the direct result of another.

Bureaucratic Politics

The limitations on the power of top political authorities have been thoroughly documented by what has perhaps been the dominant strand of political science studies of weapons programmes, dominant at least at the 'case study' level. This is the 'bureaucratic politics' approach. These studies emphasize that states are not unitary actors. They are complex ensembles of often sharply divided organizations. Policy is not decision , with that term's connotation of the formulation of goals and then rational choice of the means to fulfil those goals, but outcome, the often internally contradictory result of multiple and repeated contest.[49]

A bureaucratic politics explanation accords well with much of the history of FBM technology. Throughout there has been the pervasive significance for the FBM programme of conflict between the Navy and the Air Force (or, to be more exact, of the studious avoidance of such conflict). It is clear that 'the Navy' itself is not unitary, and that conflicts within it

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48. Interview.

49. A classic account is G. T. Allison, Essence of Decision: Explaining the Cuban Missile Crisis (Boston: Little, Brown & Co., 1971). For its application to weapons development, see G. T. Allison and F. A. Morris, 'Armaments and Arms Control: Exploring the Determinants of Military Weapons', Daedalus, Vol. 104 (Summer 1975), 99-129.

(e.g. between SPO and Rickover, or SPO and the Great Circle Group) have influenced FBM programmes. Furthermore, sub-units such as SPO can themselves be disaggregated, and found to contain technologically-important tensions, for example those between SP-23 (Guidance and Fire Control) and SP-24 (Navigation). Further disaggregation is no doubt possible, but since we are here dealing with sub-sub-units with staff numbering in the dozens rather than hundreds we are already close to the level of individuals.

One example from the history of FBM technology is the October 1971 'decision' by President Nixon to accelerate FBM submarine construction as a 'bargaining chip' for SALT. What at first sight might seem a rational decision based on international politics was in fact the outcome of the interactions of the various parts of 'the bureaucracy'. Initially the idea came from Paul Nitze, the Secretary of Defense's representative at the SALT negotiations. Nitze - always of a somewhat 'hawkish' disposition - was becoming increasingly annoyed at the way Kissinger manipulated Presidential access, and worried about what he saw as Kissinger's tendency to make concessions in order to get a deal.[50] On noting that the Soviets delegates at SALT seemed especially concerned about US plans to replace Polaris, Nitze reported back to Secretary of Defense Laird and his deputy Packard that a decision to proceed with FBM submarine construction 'would give the United States considerable leverage at the talks'.[51] Nixon, it seems, was persuaded of this 'bargaining chip' rationale, but White House staff generally favoured building more of the existing FBM submarine design rather than accelerating the development of the proposed ULMS submarine. To Kissinger, however, the bargaining chip was something to be used straightaway, not against the Soviet Union, but against Admiral Moorer, Chairman of the Joint Chiefs of Staff. The SALT talks were stalemated on the issue of limits for FBM submarines, which Kissinger was willing to concede the Soviet Union should be allowed more of because of their lesser capability (particularly compared to the MIRVed Poseidon). To make this palatable to Admiral

50. Elmo R. Zumwalt, Jr. On Watch (New York: Quadrangle, 1976), 490.
51. Ibid, 154.

Moorer and gain service support Kissinger agreed to accelerate US submarine construction at the same time.[52]

But then Packard worked with factions within the Navy (such as Admiral Rickover's office) to exclude Kissinger from the final choice. To avoid the possibility of simply constructing more submarines of the existing type (which some White House staff were known to favour), OSD carried out studies which were prepared so as to show that ULMS acceleration was the best choice.[53] The White House was not consulted and when Secretary of Defense Laird announced the decision to accelerate ULMS it was, it seems, *without* President Nixon's explicit agreement. This was a satisfactory result for many parts of the Navy (but not SPO, of course) and for people in OSD who were sick of the interference of the Kissinger-instigated Defense Program Review Committee. The outcome, however, was felt by many to be of dubious value as a bargaining chip, and to also be questionable as the best choice for the future of the FBM force. Moreover the decision to agree to the accelerated schedule (in order to make the ULMS option appear competitive with building more Poseidon-type FBM submarines or converting existing attack submarines) was to cause the programme great embarrassment in the future.

It would be hard to explain this episode without reference to the organizational wranglings of 'bureaucratic politics'. However, particular formulations of the bureaucratic politics approach have been rightly criticized.[54] It is clearly without foundation, empirical or theoretical, to treat organizations as unitary. If 'America' must be disaggregated, so must 'the Navy'. Nor are particular individuals the predictable products of their organizational location. Formal hierarchical authority, that of the Secretary of Defense or President, is not unimportant, even though it is only one political resource amongst several. Private corporations, as well as state bodies, are important - for example the important role in FBM guidance decisions played by the activities of Kearfott, and the influence of Lockheed as missile contractor for every generation of FBM is obviously

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52. John Newhouse, Cold Dawn: The Story of SALT (New York: Holt, Rinehart and Winston, 1973), 246.
53. See page 142.
54. See M. R. Sidrow, 'Politics and Military Weapons Acquisition: The Limits of Bureaucratic Political Theory' (PhD Thesis, University of California, Riverside, 1983).

important, though hard to assess precisely. The external world - what 'The Russians actually do'- is not irrelevant, however important are the bureaucratic processes by which crucial data, such as in this case that about Soviet anti-submarine warfare and anti-ballistic missile capabilities, are processed.

Graeme Allison's classic study of the Cuban Missile Crisis - in attempting to demonstrate the model for a very 'hard' case - has come in for particular criticism.[55] These critiques argue that organizational process and bureaucratic politics[56] are often of only peripheral interest, and that *real* decision-making happens at a higher level. Thus in the case-studies of Allison (Cuban Missile Crisis) or Halperin[57] (ABM decision) it is clear that the President was a very important figure, and that the behaviour of many key actors could not be explained in terms of their organizational affiliations. Allison's popular aphorism - 'Where you sit is where you stand' - seemed frequently to be wrong.[58]

Nonetheless, the essential insights of the bureaucratic politics approach - that policy and weapons procurement should be viewed as the *outcome* of social interactions in which rational argument is only one factor - are confirmed by this study. Most of the 'criticisms' -justified as this case also shows them to be - apply more to later, unimaginative and over-rigid versions than to the approach's theoretical foundations in the work, for example, of Simon on 'bounded rationality'.[59]

Moreover, a distinction should also be made between the development of policy and that of technology. An important policy

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55. D. J. Ball, 'The Blind Men and the Elephant: A Critique of Bureaucratic Politics Theory', Australian Outlook, Vol. 28 (1974), 71-92: S. D. Krasner, 'Are Bureaucracies important? (Or Allison Wonderland)', Foreign Policy, Vol. 7 (Summer 1972), 159-79.

56. Allison discusses these separately as two different models to contrast with the rational actor one, but clearly they can be combined as in G. T. Allison and M. H. Halperin, 'Bureaucratic Politics: A Paradigm and Some Policy Implication', World Politics, Vol. 24 (Spring 1972), 40-79.

57. M. H. Halperin, 'The Decision to Deploy the ABM: Bureaucratic and Domestic Politics in the Johnson Administraion', World Politics, Vol. 25 (October 1972), 62-95.

58. See Ball, 77 & 83.

59. See, for example, H. Simon, 'A Behavioral Model of Rational Choice', Quarterly Journal of Economics Vol. 69 (1955), 99-118. One application of Simon's insights to a weapons programme is J. Steinbruner, The Cybernetic Theory of Decision (Princeton, NJ: Princeton University Press, 1974).

decision *can* be made overnight, and is thus much more susceptible to 'top-down' command. A group of individual leaders - such as the members of the US Executive Committee in the Cuban Missile Crisis - can reach decisions which go against their organizational affiliations (if they indeed have any). When it comes to implementation the policy must (except in very exceptional cases) pass through the relevant bureaucracies, and then organizational interests and preferences may take affect. If the policy demands instant action then there will limited scope for alteration, but if it sets in motion a long-term process bureaucratic politics inevitably becomes more influential. Modern weapons technology, of course, may take a decade or more to develop and during this time will be repeatedly subjected to the influence of bureaucratic interactions.

And, of course, the weapons procurement process is to a large extent the realm of military organizations, in which the uniformed military provide an unusual degree of consistency. Here doctrinal uniformity and organizational loyalty do tend to produce predictable allegiances. Whilst politicians and civilian appointees may be less the product of their bureucratic affiliation, they are also likely to be less important in shaping a weapons technology over the full term of its development. They are not, as this study shows, unimportant, but they must operate in an environment shaped by the exigencies of bureaucratic interactions.

But does this mean that politics really is in command, albeit bureaucratic politics rather than 'high' politics? The difficulty with that conclusion is that just as bureaucratic politics shapes technology, so technological change shapes bureaucratic politics. The most striking instance of this concerns the military services' changing strategic preferences. The correlation we have noticed - the Air Force being pro counterforce, the Navy pro assured destruction - was in fact fairly new. In an earlier dispute in the 1940s the positions adopted had been opposite:

> In urging the procurement of the new long-range B-36 bomber, which would necessarily have limited bombing accuracy, the Air Force was advocating a policy of using nuclear weapons against large urban areas in time of war. The Navy challenged both the effectiveness and morality of this strategy and offered as an alternative increased

> reliance on carrier-based aircraft to pinpoint and deliver nuclear and conventional weapons to military targets.[60]

What brought about the change, from the Navy side, was most centrally Polaris. It was not, as we have seen, that a strategy - national or bureaucratic - was chosen and then Polaris created to fulfil its requirements. Rather, the Polaris programme was embarked on for reasons not much more specific than that missiles were the coming thing and that a coalition of interests came to support the idea that the Navy had to have one. Only as Polaris took shape did the realization evolve that 'assured destruction' was the strategy that best justified it.

Technology and Politics - 'The Seamless Web'

It would seem, then, that analysis which begins with 'technology', leads towards 'politics'; and that which starts with 'politics', leads to 'technology'. Often, indeed, it becomes difficult to distinguish the two. Is the diameter of a missile launch tube, for example, a 'political' or a 'technological' matter?

The actors involved, it turns out, *can* answer that question: they, in their interviews, routinely distinguished 'technical' decisons about technology from 'political' decisions about technology. However, that dichotomy does not capture at all well what they *do*, as distinct from what they *say* about what they do. In their activity, for example in the design decisions they take, 'technology' and 'politics' are interwoven. These engineers are indeed heterogeneous engineers.[61] Further, they do not always apply the 'technical'/'political' dichotomy consensually. One person's 'technical' decision is another's 'political' one. This is in large part because one major criterion of distinction they use is whether or not they agree with the decision. A good decision is typically regarded as 'technical'; a bad one as 'political'. Thus the decision to move from the C4's vidicon to the D5's charge-coupled device was described as 'political' (intended to enhance the authority of the Draper Laboratory within the

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60. Harvey M. Sapolsky, The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972), 5-6.
61. J. Law, 'Technology and Heterogeneous Engineering: The Case of Portuguese Expansion', in Bijker, Hughes and Pinch, 111-34.

design process) by an interviewee who disagreed with it. That characterization would, of course, be challenged by someone who agreed with the decision.

The historical or sociological analyst cannot take sides in this kind of dispute. It is not simply that we are incompetent to classify such decisions as right or wrong. More importantly, satisfactory explanation requires impartiality and symmetry in this respect.[62] We need to seek equivalent explanations of 'good' and 'bad' decisions and not, as is very common in the social science literature on technology, seek 'political' explanations only for bad decisions. So the actors' distinction between 'the technical' and 'the political' is one the analyst cannot adopt.

The literature of the 'new' sociology of technology - and indeed most recent history of technology - has, however, a simple answer to the apparent problem. The distinction is quite unnecessary, it argues. There is a 'seamless web' in which 'the technical', 'the social', 'the economic', 'the political' are inextricably interwoven.[63]

This conclusion is wholly in line with the findings of this study. It shows that the polar 'technology-out-of-control' and 'politics-in-command' views fail; that 'technology' and 'politics' interact; indeed that 'interact' is too weak a word for their labyrinthine interconnections and intermingling. In this respect the social construction of technology (SCOT) approach advocated by Pinch and Bijker seems to be inadequate.[64] Technology cannot be considered simply the product of social relations, as that approach implies. Technology is a part of social relations.

It should not be surprising then that attempts to understand technical developments in monocausal terms are doomed to failure. Such attempts deny the complexity of the seamless web. At the same time of course such a conclusion has only limited value. It serves as a reminder that technology should not be considered simplistically as either product or cause alone, and that the best methodological approach to technology

62. See Pinch and Bijker,.

63. T. P. Hughes, 'The Seamless Web: Technology, Science, Etcetera, Etcetera', Social Studies of Science Vol. 16 (1986), 281-92.

64. Pinch and Bijker.

studies may be that of the thorough historian. Rather than attempting to constrain empirical case studies to fit particular theories, it may be more fruitful 'to follow the actors' - both human and non-human - so as not to mask the rich tapestry in which the 'social' and 'technical' mingle.

Such studies can be useful in two ways. They can provide insight into the specific technology studied (such as FBM technology in this case) and they can perhaps allow more general patterns to be understood. Thus it is clear that the seamless web is not shapeless. There are patterns which can be usefully recognized, such as in the nature of weapons programmes. Here it is clear why 'technology' and 'politics' are so hard to distinguish - any successful programme obviously must 'work' both technically and politically. It also suggests that a characterization of 'programmes' would help us understand the patterns in the seamless web.

Inside the Black Box

One important such characterization is the degree of 'black-boxing' of programmes. A black-box programme is one that receives input (money, time, people, instructions) from its environment and reliably processes these into output that is acceptable (a 'working' weapon system), without actors in that environment needing, or perhaps being able, to enquire into or tinker with the internal contents of the programme.

From its very inception the managers of the Fleet Ballistic Missile programme were aware of the significance of building a black box around their technology. That is to say, almost tautologically, that it is the degree of black-boxing itself which defines what is technical and what is political. In the 1950s weapons technologies were generally regarded as black boxes, which the government paid for, but did not interfere in. SPO, however, were unusually successful in keeping the FBM programme a black box, the contents of which were 'technical' and therefore of no concern to politicians. By limiting interference with its internal behaviour SPO was thus able not only to meet goals, but also (to a considerable degree) to set the goals themselves.[65] Thus technical difficulties were far greater than

[65]. In this context black-boxing and programme success seem to be intimately connected, but this is probably not a universal connection - early post-1945 Soviet weapons programmes

the FBM's reputation would suggest (as, for example, most of the early FBM warheads had considerable reliability problems). But these difficulties were kept inside the black box.

The decisions of FBM managers, 'technical' and 'political' alike, have been shaped by the perceived exigencies of black-boxing. FBM programme managers have known that the worst thing they could do would be to take on a performance requirement and then be seen to fail to meet it. So for Polaris they made sure, as Sapolsky puts it, that: 'Performance was a manipulatable variable'[66] - so increasing the chances of success by keeping at least a degree of control within the black box over the criteria of what was to count as a 'working' system. Gradually they have been pushed towards formal requirements, especially for accuracy, but they have taken these on only as their confidence in meeting them has grown. They have thus avoided dangerous 'overcompliance' with the demands of their environment, while simultaneously avoiding too overt 'undercompliance'. Where necessary they have compromised, as with the selection of the C4 warhead which they believed to be an unnecessary expense. Great attention has been given to avoiding generating Congressional or bureaucratic enemies who might seek to open the black box. So the C4's specification was in part shaped to avoid giving offence within Congress, while relations to the Air Force have been the subtext of much of the history of FBM accuracy - accuracy being deprioritized when its pursuit might have sparked interservice rivalry, and being prioritized when the troubles of the Air Force's MX made a counterforce Trident seem widely desirable.

Successful black-boxing has important consequences. A black-boxed programme creates a boundary between 'technology' and 'politics'. This, along with the criterion of agreement or disagreement, is another way that the culture we have been studying distinguishes the 'technical' from the 'political'. What is inside the black box is 'technical'; what is outside is 'political'. Separate spheres of responsibility are thereby created. So long

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were far from black boxes as far as Party leaders were concerned, but were remarkably successful nonetheless. See David Holloway, 'Innovation in the Defence Sector: Battletanks and ICBMs", in R. Amann and J. Cooper (ed.), Industrial Innovation in the Soviet Union (New Haven, Conn.: Yale University Press, 1982), 368-414.
66. Sapolsky, 141.

as the desired output is smoothly produced, programme managers, their contractors and advisors, make decisions within the black box. The formal political system - the Secretary of Defense, President, Congress - are then presented with appropriately simple decisions, between 'buying' the package, the black box, or rejecting it. They are not troubled by 'technical detail'. But when programmes are no longer seen as a black box - such as in the MX development - then 'technical' issues can become 'political' and success elusive.

This separation of 'technology' and 'politics' is of course a production. Very careful shaping of 'technical' decisions may be needed to keep the black box shut and so maintain the separation. But if it is successful this shaping will be structural rather than overt. What we will not find is controversy, with different 'political' interests aligned behind different 'technical' options.[67] Rather, we will see a series of apparently smooth, entirely 'technical' decisions -what some analysts of technical change would refer to as a 'trajectory'.

Whether the black box can be kept shut is not, however, a matter entirely, or probably even largely, within the control of programme managers. Certainly it has become an ever more difficult task for the FBM programme. With Secretary McNamara in the early 1960s the Department of Defense became more interventionist, as did Congress with the Anti-Ballistic Missile debate of the late 1960s and the entanglement in Vietnam. Although the Department of Defense is now perhaps less interventionist, Congress may sometimes even be more so. 'Micromanagement' of programmes by the formal political system [this actors' term roughly means the opposite of black-boxing] has grown. Whereas once Admiral Raborn alone was able to 'keep the outside world at bay' by the early 1980s both the Director and Technical Director had to spend most of their time doing so.[68] Political interference is not an inevitability - the Trident programme has escaped it in any significant degree - but the recent history of MX, with the detailed interpretation of flight test results being openly debated between the House of Representatives Armed Services Committee

67. See the debate between Pinch and Bijker and Russell.
68. Interview.

and the Air Force, shows how deep within the black box it is now legitimate to go.[69]

The ability to maintain a black box also depends on other things, which although susceptible to the heterogeneous engineering of programme managers, are not completely in their control. The visibility of the technology - as in, say, the basing of US FBM submarines at Holy Loch in Scotland - can raise its public profile, as can determined opposition (or as in the case of improving Poseidon accuracy, over-zealous advocacy). Public relations is by no means an irrelevant skill for a weapons manager. Likewise, expectation can sometimes exceed achievement by such a margin that the programme manager's competence may be doubted. Despite the best efforts of programme managers to shape the world so that it does not happen, test missiles do, for example, blow up or have to be blown up. 'Technical' failure is of course always negotiable: one person's 'failed' test is another's 'partial success'.[70] And its consequences within a black-boxed programme are containable. The ninth flight test of Trident D5, on 21 January 1988, which was terminated in an explosion initiated by the Range Safety Officer,[71] will do the programme no harm, while what might well be seen as smaller troubles in the MX test programme have been seized upon in the formal political system. Much more serious 'technical' difficulties (for example, with Poseidon's re-entry vehicles and electronics or Trident C4's rocket motors, and with Polaris and Poseidon warhead designs) have been contained within the black box in previous FBM programmes.

Nevertheless, that programmes require successful manipulation of the physical world, and that this world is not wholly within managers' control, is a feature of some importance in regard to black-boxing. The reason this particular feature is so hard to specify is of course again the interweaving of the physical and the social. This is most evident in a

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69. See the report by that Committee's Subcommittee on Research and Development, and Procurement and Military Nuclear Systems, The MX Inertial Measurement Unit: A Program Review (Washington, DC: US Government Printing Office, 1987).

70. On missile testing, see D. MacKenzie, 'From Kwajalein to Armageddon? Testing and the Social Construction of Missile Accuracy', forthcoming in D. Gooding, T. Pinch and S. Schaffer (eds.), The Uses of Experiment: Studies of Experiment in the Natural Sciences (Cambridge: Cambridge University Press, 1988).

71. The Guardian (22 January, 1988), 7.

vitally important aspect of weapons programmes almost completely neglected in the political science literature - that these programmes involve material processes of production. Several of the components of missiles - notably solid rocket motors, re-entry vehicle casings and inertial sensors together with some other guidance system parts - are amongst the most difficult of human products, involving highly skilled 'art-like' production processes within eliminable elements of 'tacit knowledge'.[72]

'Producibility' - which we have seen in the case of Honeywell and the electrostatic gyroscope to include 'social' as well as 'physical' elements -has been a major factor in decisions on items such as inertial components for the FBM programmes. Although there have been problems in the production of the Mk6 guidance system for Trident D5 - notably with the PIGA accelerometer and charge-coupled device stellar sensor - these have been contained in their effects, and have not endangered the programme.

Practical Implications

Adopting a position of empirical relativism to the study of technology is useful because it helps reveal the social processes at work in the creation of technological artefacts and of knowledge about them. However, it should not blind analysts into thinking that technology can be considered to be *totally* socially constructed, as if the physical world were irrelevant, or into adopting a position of political relativism. The development of technology is necessarily a social process because it is a part of human society, and our knowledge of technology is socially-mediated. Yet it would be absurd to suggest that this means that no physical reality is involved, or that technology works only because we believe it to. What exactly we take to have happened at Hiroshima and Nagasaki is, like all history, a social construct. But it seems indisputable that despite their lack of understanding of how an atomic weapon might work, the unfortunate inhabitants of those cities did indeed suffer the terrible consequences of what was a very public demonstration of an awesome new technology working.

72. On tacit knowledge see H. M. Collins, 'The TEA Set: Tacit Knowledge and Scientific Networks', Science Studies, Vol. 4 (1974), 165-86.

In stressing the social nature of technology, and its underdetermination by the physical world, it is important not to lose sight of the very real, and too often, disast-rous effects it can have. Nuclear weapons may be socially-shaped artefacts, our understanding of which is socially-created, but they also could destroy the world (and all the sociologists therein). It is thus important to attempt to view these theoretical perspectives in a practical light which may have some utility. In what ways does it help us to write social histories of nuclear weapons technology?

To begin with, it is clear that such studies discredit simple technological determinism and the fatalistic attitudes that it implies. There is not an inevitable, internal dynamic to weapons technology which is leading us towards the nuclear holocaust. Instead it is clear from this study (and many others) that nuclear weapons technologies are, like other technologies, both a product and a part of society.

In avoiding fatalistic and apathetic attitudes to technology it is important to get inside the black box, and reveal what goes on inside. Only then can we observe how apparently 'natural' trajectories are social in nature. Only then can the possibilities for intervention in the technological process be fully comprehended. Detailed studies of particular technological developments are thus essential if there is to be any attempt at shaping their progress. Without such studies the technology will remain opaque, black-boxed to outsiders, and apparently inevitable.

Because if technology is not completely out-of-control, neither is it very much under control (at least not by most of us). There are powerful vested interests involved in the development of weapons technology, and their power in part stems from their ability to delineate the bounderies of the black box. Institutional and economic interests push very strongly towards 'follow-on' weapons systems, as does explicit governmental policy aimed at maintaining specialized research teams. All this, however, is a social process which can, and often does, involve many actors. Ignorance of the process will not only allow it to appear as an inevitable case of 'follow-on', but it will in the end most likely be self-fulfilling.

Those interested in continuing weapons succession are so for generally sound reasons. Corporate managers are, of course, primarily concerned with the financial well-being of their company. Those in the Department of Defense and in the armed services are focused on the military means of international relations, as well as on their own particular organizational loyalties. In the Administration and Congress too, questions of weapons technology are largely, and not surprisingly, left to those most imbued with that particular culture. In this process fraud and mismanagement do undoubtedly exist, but are probably not especially significant. The weapons succession process goes on, not because of an internal technological imperative, but because those involved are too often unchecked by those (the rest of us) who are not.

This is not the place to argue what the nature of US nuclear weapons procurement policy should be. There is, of course, the difficulty of deciding whether weapons technology simply meets the need to deter the threat, whether it is excessive, or indeed whether it might be inadequate. To many the FBM programme has always seemed the 'ideal' nuclear deterrent. The combination of invulnerability without hard target capability seemed to make the FBM system a (relatively) non-threatening, last-resort, retaliation-only deterrent. With Trident II, however, the FBM's status is no longer clear. To many the move to hard-target counterforce seems either dangerous, or at best, unnecessary and expensive.

On past experience a successor to Trident II would be expected to be deployed sometime around 2000.[73] That would probably mean that various follow-on proposals will be put forward before the end of the 1980s. Indeed FBM Steering Task Group meetings in 1987 were considering what characteristics another FBM generation might have.[74] But perhaps a new era in American-Soviet relations and a new American President will build upon the example of the Intermediate Nuclear Forces Treaty to reduce the importance of nuclear weapons in international

73. IOCs of Poseidon, Trident I and Trident II have been 1971, 1979 and 1989, giving intervals of 8 and 10 years.
74. Interview.

relations. Whether another FBM generation will then be sustainable, and if so, how, may tell us more about the weapons succession process. Meanwhile, however, those who doubt such a need should not wait too long to see what a Trident III might look like. Whilst they are waiting SSPO, Lockheed and many others will be doing what they have always done well - heterogeneous engineering.

Bibliography

Books and Monographs

Aldridge, Robert C. First Strike: The Pentagon's Strategy for Nuclear War (London: Pluto Press, 1984).

Allison, G. T. Essence of Decision: Explaining the Cuban Missile Crisis (Boston: Little, Brown & Co., 1971).

Applied Physics Laboratory, The First Forty Years (Silver Spring, Maryland: Johns Hopkins University Applied Physics Laboratory, 1983).

Armacost, Michael H. The Politics of Weapons Innovation: The Thor-Jupiter Controversy (New York: Columbia University Press, 1969).

Baar, James and William E. Howard. Polaris! (New York: Harcourt, Brace & World, Inc., 1960).

Ball, Desmond. Can Nuclear War Be Controlled (London: International Institute for Strategic Studies, Adelphi Paper #169, 1981).

Ball, Desmond. Deja Vu: The Return to Counterforce in the Nixon Administration
nta Monica, (California: Seminar on Arms Control and Foreign Policy, 1974).

Ball, Desmond. Politics and Force Levels: The Strategic Missile Program of the Kennedy Administration (Berkeley: University of California Press, 1980).

Ball, Desmond. Targeting for Strategic Deterrence (London: International Institute for Strategic Studies, Adelphi Paper #185, 1983).

Barnes, Barry and David Edge (eds.). Science in Context (Milton Keynes: Open University Press, 1982).

Barnes, Barry. About Science (Oxford: Basil Blackwell, 1985).

Beard, E. Developing the ICBM: A Study in Bureaucratic Politics (New York: Columbia University Press, 1976).

Bijker, Wiebe E., Thomas P. Hughes and Trevor Pinch (eds.). The Social Construction of Technological Systems (Cambridge, Mass.: MIT Press, 1987).

Blair, Bruce G. Strategic Command and Control: Redefining the Nuclear Threat (Washington, DC: Brookings Institution, 1985).

Bunn, Matthew. Technology of Ballistic Missile Reentry Vehicles (Cambridge, Mass.: MIT Program in Science and Technology for International Security, Report #11, March 1984).

Canan, James W. The Superwarriors: The Fantastic World of Pentagon Superweapons (New York: Weybright and Talley, 1975).

Carter, Ashton B., John D. Steinbruner and Charles A. Zraket. Managing Nuclear Operations (Washington, DC: Brookings Institution, 1987).

Cochran, T. B., W. M. Arkin and M. M. Hoenig. Nuclear Weapons Databook - Volume I: US Nuclear Forces and Capabilities (Cambridge, Mass.: Ballinger, 1984).

Cochran, T. B., W. M. Arkin, R. S. Norris and M.M. Hoenig, Nuclear Weapons Databook Volume II: US Nuclear Warhead Production (Cambridge, Mass.: Ballinger, 1987).

Dalgleish, D. Douglas and Larry Schweikart. Trident (Carbondale, Il.: Southern Illinois University Press, 1984).

Daniel, Donald C. Anti-submarine Warfare and Superpower Strategic Stability (London: Macmillan, 1986).

Davis, Vincent. 'The Politics of Innovation: Patterns in Navy Cases', Monograph Series in World Affairs, Vol. 4, No. 3 (Denver, Colo.: University of Denver, 1966-67).

Dicerto, J. J. Missile Base Beneath the Sea: The Story of Polaris (New York: St. Martin's Press, 1967).

Edwards, J. Superweapon: The Making of MX (New York: Norton, 1982).

Elliott, Brian (ed.). Technology and Social Process (Edinburgh: Edinburgh University Press, 1988).

Emme, E. M. (ed.). The History of Rocket Technology: Essays on Research, Development and Utility (Wayne State University Press, 1964).
roit, Mich.

Enthoven, Alain C. and K. Wayne Smith. How Much Is Enough? Shaping the Defense Program, 1961-1969 (New York: Harper & Row, 1971).

Gansler, Jacques S. The Defense Industry (Cambridge, Mass.: MIT Press, 1980).

Greenwood, Ted. Making the MIRV: A Study of Defense Decision Making (Cambridge, Mass.: Ballinger, 1975).

Gregory, Shaun. The Command and Control of British Nuclear Weapons (Bradford: University of Bradford, School of Peace Studies, Peace Research Report #13, December 1986).

Halliday, Fred. The Making of the Second Cold War (London: Verso, 1983).

Hansen, Chuck US Nuclear Weapons: The Secret History (Arlington, Texas: Aerofax, 1988).

Hewlett, Richard and Francis Duncan. Nuclear Navy (Chicago: University of Chicago Press, 1974).

Hughes, T. P. Networks of Power: Electrification in Western Society (London and Baltimore, MD.: The Johns Hopkins University Press, 1983).

Kaldor, Mary The Baroque Arsenal (London: Andre Deutsch, 1982).

Kaplan, Fred. The Wizards of Armageddon (New York: Simon and Schuster, 1983).

Klessig, Lowell L. and Victor L. Strite. The ELF Odyssey: National Security Versus Environmental Protection (Boulder, Colo.: Westview Press, 1980).

Koppers, Clayton R. JPL and the American Space Program (New Haven, Conn.: Yale University Press, 1982).

Lapp, Ralph E. Arms Beyond Doubt: The Tyranny of Weapons Technology (New York: Cowles Book Company, Inc., 1970).

Latour, B. Science in Action (Milton Keynes: Open University Press, 1987).

Long, Franklin A. and George W. Rathjens (eds.). Arms, Defense Policy, and Arms Control (New York: Norton, 1976).

MacKenzie, Donald and Judy Wajcman (eds.). The Social Shaping of Technology (Milton Keynes: Open University Press, 1985).

Melman, Seymour. Pentagon Capitalism: The Political Economy of War (New York: McGraw-Hill Book Company, 1970).

Newhouse, John. Cold Dawn: The Story of SALT (New York: Holt, Rinehart and Winston, 1973).

Perry, Robert L. The Ballistic Missile Decisions (Santa Monica, Calif.: The Rand Corporation, P-3686, 1967).

Perry, Robert. 'The Interaction of Technology and Doctrine in the USAF' (Santa Monica, Calif.: The Rand Corporation, P-6281, January 1979).

Polmar, Norman and Thomas Allen. Rickover (New York: Simon and Schuster, 1982).

Pranger, Robert J. and Roger P. Labrie (eds.). Nuclear Strategy and National Security: Points of View (Washington, DC: American Enterprise Institute for Public Policy Research, 1977).

Rasor, Dina (ed.). More Bucks, Less Bang: how the Pentagon Buys Ineffective Weapons (Washington, DC: Fund for Constitutional Government, 1983).

Rees, Ed. The Sea and the Subs (New York: Duell, Sloan and Pearce, 1961).

Rosen, Steven (ed.) Testing the Theory of the Military-Industrial Complex (Lexington, Mass.: Lexington Books, 1973).

Sapolsky, Harvey M. The Polaris System Development: Bureaucratic and Programmatic Success in Government (Cambridge, Mass.: Harvard University Press, 1972).

Schroeer, Dietrich. Science, Technology and the Nuclear Arms Race (New York: Wiley, 1984).

Scoville, H., Jr. MX: Prescription for Disaster (Cambridge, Mass.: MIT Press, 1981).

Smith, Merritt Roe (ed), Military Enterprise and Technological Change (Cambridge, Mass.: MIT Press, 1985).

Steinbruner, J. The Cybernetic Theory of Decision (Princeton, NJ: Princeton University Press, 1974).

Tammen, R. L. MIRV and the Arms Race: An Interpretation of Defense Strategy (New York: Praeger, 1973).

Thee, Marek. Military Technology, Military Strategy and the Arms Race (London: Croom Helm, 1986).

Tyler, Patrick. Running Critical: The Silent War, Rickover, and General Dynamics (New York: Harper & Row, 1986).

Wilkes, Owen and Nils Petter Gleditsch. Loran-C and Omega: A study of the military importance of radio navigation aids (Oslo: Norwegian University Press, 1987).

Winner, Langdon. Autonomous Technology: Technics-out-of-Control as a Theme in Political Thought (Cambridge, Mass.: MIT Press, 1977).

York, Herbert. Race to Oblivion: A Participant's View of the Arms Race (New York: Simon and Schuster, 1970).

York, Herbert. The Origins of MIRV (Stockholm: SIPRI Research Report #9, August 1973).

Zumwalt, Elmo R., Jr. On Watch (New York: Quadrangle, 1976).

Papers and Articles

Allison, G.T. and M. H. Halperin. 'Bureaucratic Politics: A Paradigm and some Policy Implications', World Politics, Vol 24 (1972), 40-79.

Allison, Graham T. and Frederic A. Morris. 'Armaments and Arms Control: Exploring the Determinants of Military Weapons', Daedalus, Vol. 104 (Summer 1975), 99-129 ; reprinted in Franklin A. Long and George W. Rathjens (eds.). Arms, Defense Policy, and Arms Control (New York: Norton, 1976), 99-129.

Anderle, R. J. 'Error Model for Geodetic Positions Derived from Doppler Satellite Observations', Bull. Geod., Vol. 50 (1976), 43-77.

Anderton, D. A. 'Loran-C Extension Proposed for Tracking', Aviation Week and Space Technology (October 1, 1962), 41-47.

Anon. 'Aerojet Methods Help Boost Polaris Range', Missiles and Rockets, Vol. 12 (February 25, 1963), 35.

Anon. 'Anti-Satellite Polaris Being Developed', Aviation Week & Space Technology, Vol. 81 (September 28, 1964), 18-19.

Anon. 'Bigger, Better Subs Are Coming', Missiles and Rockets, Vol. 7 (July 25, 1960), 31.

Anon. 'Boeing, Lockheed Submit Tacamo Successor Plans', Aviation Week & Space Technology, Vol. 112 (April 14, 1980), 18.

Anon. 'Compressed Air to Shoot Polaris From Submarine to Surface', Aviation Week, Vol. 68 (April 21, 1958), 31.

Anon. 'First Polaris A3 Test Is Partly Successful', Aviation Week & Space Technology, Vol. 77 (August 13, 1962), 29.

Anon. 'Funds Urged for ULMS, Subs Opposed', Aviation Week & Space Technology, Vol. 96 (April 17, 1972), 18.

Anon. 'Gradual Poseidon Modification Planned', Aviation Week & Space Technology, Vol. 99 (September 17, 1973), 19.

Anon. 'Graphite Chamber Developed for Trident 2', Aviation Week & Space Technology, Vol 117 (August 30, 1982).

Anon. 'Hercules, Thiokol Win Poseidon Work', Missiles and Rockets, Vol. 17 (October 25, 1965), 16.

Anon. 'Joint Study Weighs Common Missile', Aviation Week & Space Technology Vol. 108 (June 19, 1978), 23.

Anon. 'LMSC - Missile Systems Division', Lockheed Horizons, Issue 12 (June 7, 1983), 79-91.

Anon. 'Lockheed Polaris A-3 Tests Slated', Aviation Week & Space Technology, Vol. 76 (April 2, 1962), 23.

Anon. 'Longer Range Is Goal of Polaris B3', Aviation Week & Space Technology, Vol. 78 (March 11, 1963), 145.

Anon. 'Longer Range Promised Through Improved Motors', Polaris Special Report, Missiles and Rockets, Vol. 7 (July 25, 1960), 20-3.

Anon. 'McNamara Says DOD Is Pushing Polaris B3', Missiles and Rockets, Vol. 14 (February 24, 1964), 16.

Anon. 'Minority Report Asks Trident Stretch', Aviation Week & Space Technology, Vol. 99 (September 17, 1973), 20.

Anon. 'More 1958 Money Planned for Polaris', Aviation Week, Vol. 68 (January 6, 1958), 39.

Anon. 'Navy Adopts Long-Range Spending Plan to Enhance Capabilities', Aviation Week & Space Technology Vol. 123 (December 9, 1985), 75.

Anon. 'Navy Asks Evolution Of 2,500 mi. Polaris', Aviation Week & Space Technology, Vol. 73 (August 1, 1960), 27.

Anon. 'Navy Budgets Trident R&D, Sets 1989 Operation Start', Aviation Week & Space Technology, Vol. 121 (September 3, 1984), 297, 299.

Anon. 'Navy Considers Polaris Follow-on', Aviation Week & Space Technology, Vol. 76 (April 9, 1962).

Anon. 'Navy Funds Trident Replacement Study', Aviation Week & Space Technology, Vol. 110 (April 30, 1979), 193-4.

Anon. 'Navy Launches First Advanced Trident Missile', Aviation Week & Space Technology Vol. 126 (January 19, 1987), 29.

Anon. 'Navy Plans Development Program For Polaris High-Yield Warhead', Aviation Week, Vol. 70 (March 2, 1959), 43-4.

Anon. 'Navy Poseidon Payload Will Double', Aviation Week & Space Technology, Vol. 82 (March 15, 1965), 142.

Anon. 'Navy Quits Joint IRBM Project, Confirms Plans for Own Missile', Aviation Week, Vol. 66 (January 7, 1957), 32.

Anon. 'Navy Secretary Considers Alternative to Trident Sub', Aviation Week & Space Technology, Vol. 114 (March 30, 1981), 64.

Anon. 'Navy Seeks Communications Link for Trident Missile Force', Aviation Week & Space Technology Vol. 112 (June 16, 1980), 113-15.

Anon. 'Navy Shifts Trident Management Focus', Aviation Week & Space Technology Vol. 112 (June 16, 1980), 111-13.

Anon. 'Navy Standardizes Polaris SINS System', Aviation Week & Space Technology, Vol. 80 (May 4, 1964), 16.

Anon. 'Navy to Develop New Trident Warhead', Aviation Week & Space Technology Vol. 118 (January 17, 1983), 26.

Anon. 'Navy Views Polaris as Support Weapon', Aviation Week, Vol. 66 (June 17, 1957), 31.

Anon. 'New Missile Split', Aviation Week, Vol. 64 (February 27, 1956), 34.

Anon. 'Polaris A-3 Launch Tube Production at Westinghouse: A Step-by-Step Report', Missiles and Rockets, Vol. 15 (December 7, 1964), 14-5.

Anon. 'Polaris A3 Firing Tests Under Way', Missiles and Rockets, Vol. 11 (August 13, 1962), 15.

Anon. 'Polaris Begins Underwater Tests', Aviation Week, Vol. 67 (November 4, 1957), 34.

Anon. 'Polaris Development, Testing Accelerated', Aviation Week, Vol. 70 (January 19, 1959), 32-33.

Anon. 'Polaris Launcher Production Hits Four a Week', Missiles and Rockets, Vol. 10 (February 12, 1962), 32-3.

Anon. 'Polaris Navigation Aids Tested Ashore', Aviation Week, Vol. 71 (July 27, 1959), 79-80.

Anon. 'Polaris Test Vehicle Destroyed After Flying Off Planned Course', Aviation Week, Vol. 73 (August 8, 1960), 34.

Anon. '"Pop-Up" Site Saves Millions in Polaris R&D', Missiles and Rockets, Vol. 7 (July 25, 1960), 28-30.

Anon. 'Poseidon Missiles To Get More RVs - But Not 14', Aerospace Daily (October 30, 1980), 331-32.

Anon. '"Private Ocean" for Polaris', Missiles and Rockets, Vol. 8 (January 9, 1961), 16-17.

Anon. 'Problems May Cut Polaris A3 Range Goal', Aviation Week & Space Technology, Vol. 75 (September 4, 1961), 31.

Anon. 'Problems May Cut Polaris A3 Range Goal', Aviation Week & Space Technology, Vol. 75 (September 4, 1961), 31.

Anon. 'Seafarer Fund Drive Spurred by Navy', Aviation Week & Space Technology Vol. 106 (April 4, 1977), 51-54.

Anon. 'Specified Range Forecast for Trident', Aviation Week & Space Technology, Vol. 102 (May 5, 1975), 55.

Anon. 'Submerged Sub Fires Polaris A-3', Aviation Week & Space Technology, Vol. 79 (November 4, 1963), 32.

Anon. 'The Poseidon Misadventure', Business Week (October 13, 1973).

Anon. 'Trident Missile Capabilities Advance', Aviation Week & Space Technology Vol. 112 (June 16, 1980), 91-100.

Anon. 'Trident Subsystem Tests in Final Phase', Aviation Week & Space Technology Vol. 103 (November 3, 1975), 34-8.

Anon. 'Updated Propulsion System Seen Extending Trident Range', Aviation Week & Space Technology, Vol. 98 (June 18, 1973), 19.

Anon. 'Weather Chief Problem in Trident Tests', Aviation Week & Space Technology Vol. 107 (October 31, 1977), 46-47.

Arkin, William M. 'Sleight of hand with Trident II', Bulletin of the Atomic Scientists, Vol. 40 (December 1984), 5-6.

Arkin, William M. and Richard Fieldhouse. 'Nuclear weapon command, control and communications' in SIPRI Yearbook 1984 (London: Taylor & Francis, 1984), 455-516.

Baar, James. 'Navy Wants Huge Fleet of Missile Subs, Ships', Missiles and Rockets Vol. 5 (August 24, 1959), 36-37.

Baar, James. 'Polaris A-2 Production Starts', Missiles and Rockets, Vol. 9 (November 20, 1961), 12-3.

Backus, P. H. 'Finite Deterrence, Controlled Retaliation', United States Naval Institute Proceedings, Vol. 85 (March 1959), 22-29.

Ball, D. J. 'The Blind Men and the Elephant: A Critique of Bureaucratic Politics Theory', Australian Outlook, Vol. 28 (1974), 71-92.

Ball, D. J. 'The Counterforce Potential of American SLBM Systems', Journal of Peace Research, Vol. XIV (1977), 23-40.

Ball, D. J. 'The Role of Strategic Concepts and Doctrine in US Strategic Nuclear Force Development' in B. Brodie, M. D. Intriligator and R. Kolkowicz (eds.), National Security and International Security (Cambridge, MA: Oelgeschlager, Gunn & Hain, 1983), 37-63.

Barnes, B. 'The Science-Technology Relationship: A Model and a Query', Social Studies of Science, Vol. 12 (1982), 166-73.

Battin, R. H. 'Space Guidance Evolution - A Personal Narrative', Journal of Guidance and Control, Vol. 5 (1982), 97-110.

Boutacoff, David A. 'New Tacamo Aircraft Being Developed to Support Trident Missile Submarines', Defense Electronics, Vol. 16 (March 1985), 108-111.

Boyle, Richard. '1960: A Vintage Year for Submarines', United States Naval Institute Proceedings Vol. 96 (October 1970), 35-41.

Breasted, David C. 'Navy Seeks Approval For Polaris Follow-on', Missiles and Rockets, Vol. 13 (November 4, 1963), 18.

Brown, Neville. 'Deterrence from the sea', New Scientist (April 16, 1970), 127, 130-32.

Brownlow, Cecil. 'Major ULMS Funding Expected', Aviation Week & Space Technology, Vol. 93 (July 20, 1970), 14-15.

Burke, Gerard Keith. 'The Need for Trident', US Naval Institute Proceedings Vol. 104 (November 1978), 32-41.

Burke, Gerard Keith. 'The Trident Missile: America's Future Deterrent', US Naval Institute Proceedings Vol. 106 (March 1980), 131-34.

Burke, Gerard Keith. 'To Build Trident', US Naval Institute Proceedings Vol. 105 (October 1979), 117-120.

Burriss, Stanley W. 'The Development of an All-American Product', Navy Magazine (July 1964), 20-23.

Caligiuri, Joseph F. 'Navigation for the Polaris Submarine', reprinted from Sperry Engineering Review, Vol. 14 (June 1961).

Caligiuri, Joseph F. 'The Navigation System for the Polaris Submarine', Navigation - Journal of the Institute of Navigation, Vol 7 (Spring 1960), 3-10.

Carter, Ashton B. 'The Command and Control of Nuclear War', Scientific American, Vol. 252 (January 1985), 20-27.

Carter, Luther J. 'Seafarer: Project Still Homeless as Milliken Says No to Navy', Science, Vol. 197 (September 2, 1977), 964-68.

Cimbala, Stephan J. 'The Sea-Based Strategic Deterrent', National Defense, Vol. 405 (February 1985), 27-31.

Clautice, W. G. 'Submarine Navigation', Navigation - Journal of the Institute of Navigation, Vol. 25, No. 1 (Spring 1978), 34-39.

Collins, H. M. 'The TEA Set: Tacit Knowledge and Scientific Networks', Science Studies, Vol. 4 (1974), 165-86.

Cordesman, Anthony H. 'Reagan and Strategic Forces: The Importance of the "Devil's Alternative"', Armed Forces Journal International (April 1981), 62-83.

Dassoulas, J. 'The Triad Spacecraft', APL Technical Digest, Vol. 12, No. 2 (April-June 1973), 2-13.

Dean, W. N. and D. P. Roth, 'The AN/BRN-5 Loran receiver', Navigation - Journal of the Institute of Navigation, Vol. 23 (Winter 1976), 287-97.

Dosi, Giovanni. 'Technological Paradigms and Technological Trajectories', Research Policy, Vol. 11 (1982), 147-62.

Dulberger, Leon H. 'Computer Helps Polaris Subs Navigate' Electronics, Vol 34 (January 6, 1961), 40-1.

Dulberger, Leon H. 'Geodetic Measurements From Space', Space/Aeronautics (June 1965), 34-43.

Eliot, George Fielding. 'Defense Decisions in the Dangerous Decade', US Naval Institute Proceedings, Vol. 87 (January 1961), 22-31.

Elson, Benjamin M. 'Poseidon Checkout Computerized', Aviation Week & Space Technology, Vol. 92 (February 16, 1970), 48-9.

Freitag, Captain Robert F. 'Project Transit - Navigation Satellite', US Naval Institute Proceedings Vol. 87 (May 1961), 77-83.

Fuhrman, R. A. 'The Fleet Ballistic Missile System: Polaris to Trident', Journal of Spacecraft, Vol. 15 (Sept-Oct 1978), 265-86.

George, James L. 'The Future of US Sea-Based Missiles', Navy International, Vol. 87 (March 1982), 916-18.

Getler, Michael. 'Improved Polaris Fire-Control System Going to Sea Duty Shortly', Missiles and Rockets, Vol. 13 (November 4, 1963), 32-3.

Gray, Colin. 'Nuclear Strategy: The Case for a Theory of Victory', International Security Vol. 4 (Summer 1979).

Haines, Commander R. G. 'Eyes and ears on the nuclear patrol', New Scientist (August 28, 1969), 418-20.

Halperin, M. H. 'The Decision to Deploy the ABM: Bureaucratic and Domestic Politics in the Johnson Administraion', World Politics, Vol. 25 (October 1972), 62-95.

Homer, Frank. 'The Navy's Polaris: Awesome Power at Sea', Navy Magazine (July 1964), 7-12.

Homes, Theodore J. 'SINS: A Shipboard Inertial Navigation System', Sperry Engineering Review, Vol. 17 (Spring 1964), 35-9.

Hughes, T. P. 'Technological Momentum in History: Hydrogenation in Germany 1898-1933', Past and Present, No. 44 (1969), 106-32.

Hughes, T. P. 'The Electrification of America: The System Builders', Technology and Culture, Vol. 20, No. 1 (1979), 124-161.

Hughes, T. P. 'The Science-Technology Interaction: The Case of High-Voltage Power Transmission Systems', Technology and Culture, Vol. 17 (1976), 646-62.

Hung, J. C. and G. B. Doane, III, 'Progress in Strapdown Technology' in Inertial Navigation Components and Systems: AGARD Conference Proceedings No. 116 (papers presented at the 15th meeting of the Guidance and Control Panel of the NATO Advisory Group for Aerospace Research and Development, Florence, 2-5 October 1972).

Judge, John F. 'Polaris Cases Wound in Nebraska', Missiles and Rockets, Vol. 14 (May 4, 1964), 30, 32.

Jurgen, Ronald K. 'ELF Update', US Naval Institute Proceedings Vol. 106 (March 1980), 135-36.

Kaldor, Mary. 'Military R&D: cause or consequence of the arms race?', International Social Science Journal, Vol. 35, No. 1 (1983), 25-45.

Kershner, Richard B. 'Technical Innovations in the APL Space Department', Johns Hopkins APL Technical Digest, Vol. 2 (Jan-March 1981), 264-78.

Kershner, Richard B. 'Transit Program Results', Astronautics (May 1961),

King, T. A. and H. Strell. 'Underwater Navigation' entry in McGraw-Hill Encyclopedia of Science and Technology (New York: McGraw-Hill Publishing Company, 1982), 399-402.

Klass, P. J. 'Computer Simplifies Loran-C Navigation', Aviation Week and Space Technology Vol. 80 (15 June 1964), 95-97.

Klass, P. J. 'Inertial System uses Electronstatic Gyros', Aviation Week and Space Technology, Vol. 79 30 September 1963, 87-89.

Klass, P. J. 'Laser Communications Plan Studied', Aviation Week & Space Technology Vol. 111 (September 3, 1979), 73-77.

Klass, P. J. 'Navy to Test Electrically Suspended Gyro', Aviation Week and Space Technology , Vol. 74 (February 6, 1961), 85-91.

Klass, P. J. 'New Gyro nears Operational Use', Aviation Week and Space Technology , Vol. 96 (June 19, 1972), 50-52.

Klass, P. J. 'Pentagon Seeks Penetration Aids', Aviation Week & Space Technology Vol. 121 (September 3, 1984), 44-45.

Knoebel, H. W. 'The Electric Vacuum Gyro: Pinpoint for Polaris Launching', Control Engineering , February 1964, 70-73.

Kobayashi, W. S. and J. L. Saperstein. 'Low-Temperature Ablator Tests for Shape-Stable Nosetip Applications on Maneuvring Re-entry Vehicles' in T. E. Horton (ed.) Thermophysics of Atmospheric Entry (New York: American Institute of Aeronautics and Astronautics, Inc., 1982).

Kolcum, Edward H. 'Eastern Range Updating Timed for Trident 2 Tests', Aviation Week & Space Technology, Vol. 121 (August 27, 1984), 60, 65-66.

Kolcum, Edward H. 'First Polaris Launched From Submarine', Aviation Week, Vol. 73 (July 25, 1960), 32-3.

Kolcum, Edward H. 'Navy Improving Test Facilities For Trident 2 Missile Program', Aviation Week & Space Technology Vol. 123 (September 30, 1985), 76-79.

Kolcum, Edward H. 'Soviet Intelligence Ship Intrudes on Trident Test', Aviation Week & Space Technology, Vol. 116 (January 25, 1982), 20-22.

Kolcum, Edward H. 'U.S. Stressing New Re-entry Technology', Aviation Week & Space Technology, Vol. 86 (February 6, 1967), 26-7.

Krasner, S. D. 'Are Bureaucracies important? (Or Allison Wonderland)', Foreign Policy Vol. 7 (Summer 1972), 159-79.

Kurth, James R. 'Aerospace Production Lines and American Defense Spending', in Steven Rosen (ed.), Testing the Theory of the Military-Industrial Complex (Lexington, Mass.: Lexington Books, 1973), 135-56.

LaFond, Charles D. 'FBM Accuracy Starts with SINS', Polaris Special Report, Missiles and Rockets, Vol. 7 (July 25, 1960), 24-5.

LaFond, Charles D. 'New SINS Nears Sea Tests', Missiles and Rockets, Vol. 10 (April 30, 1962), 33.

LaFond, Charles D. 'Phase Two Poseidon Contracts Let', Missiles and Rockets, Vol. 18 (April 11, 1966), 28, 31.

Latour, B. '*The Prince* for Machines as well as for Machinations', in Brian Elliott (ed.), 20-43.

Law, J. 'On the Social Explanation of Technical Change: The Case of the Portuguese Maritime Expansion', Technology and Culture, Vol. 28, No. 2 (1987), 227-252.

Law, J. 'Technology and Heterogeneous Engineering: The Case of Portuguese Expansion', in Bijker, Hughes and Pinch (eds.), 111-34.

Leary, Frank. 'ULMS: Strategic Emphasis Shifts Seaward', Space/Aeronautics (June 1970), 24-33.

Leary, Frank. 'ULMS: Will All The Targets Go To Sea', Armed Forces Management, (May 1970), 36-40.

Lindsey, Robert. 'B-3 Polaris Expected To Be Operational in '70', Missiles and Rockets, Vol. 15 (August 24, 1964), 28, 31.

Lindsey, Robert. 'First Polaris A3 Launch Draws Near', Missiles and Rockets, Vol. 11 (July 9, 1962), 17.

Lindsey, Robert. 'Improved Launch System for Polaris', Missiles and Rockets, Vol. 13 (December 2, 1963), 27-28.

Lindsey, Robert. 'Materials Refinement Assists Poseidon Launcher Designers', Technology Week (June 13, 1966), 32-33.

Lindsey, Robert. 'Polaris A2 Parts Rejection Slashed', Missiles and Rockets, Vol. 12 (April 8, 1963), 33-34.

Lindsey, Robert. 'Proposed Fourth-Generation Polaris Would Use State-of-art Technology', Missiles and Rockets, Vol. 14 (January 13, 1964), 18, 21.

Logsdon, Tom and Charles W. Helms, 'Comparison Between the Capabilities of the Navstar GPS and other Operational Radionavigation Systems', Paper prepared for Presentation at EASCON '81, Washington, DC (Rockwell International: November 16, 1981).

Lowell, Arthur L. and Michael T. Brodsky. 'Flight Testing the US Navy's Sea-Based Ballistic Missile System', Lockheed Horizons (Spring 1981), 12-23.

MacKenzie, D. 'From Kwajalein to Armageddon? Testing and the Social Construction of Missile Accuracy', forthcoming in D. Gooding, T. Pinch and S. Schaffer (eds.), The Uses of Experiment: Studies of Experiment in the Natural Sciences (Cambridge: Cambridge University Press, 1988).

MacKenzie, D., W. Rüdig and G. Spinardi, 'Social Research on Technology and the Policy Agenda: An Example from the Strategic Arms Race', in Brian Elliott (ed.), 152-80.

MacKenzie, Donald. 'Missile Accuracy: A Case Study in the Social Processes of Technological Change', in Bijker, Hughes and Pinch (eds.), 195-222.

Marshall, Eliot. 'ELF Resurrected After Drowning by Navy', Science, Vol. 212 (May 8, 1981), 644-45.

Marshall, Eliot. 'Project ELF Finally Wins a Vote', Science, Vol. 221 (August 12, 1983), 630-31.

Martin, J. J. 'Trident's Role in National Security', Oceanus Vol. 28 (Summer 1985), 38-44.

McKelvie, B. and H. Galt, Jr. 'The Evolution of the Ship's Inertial Navigation System for the Fleet Ballistic Missile Program', Navigation: Journal of the Institute of Navigation, Vol. 25 (Fall 1978), 310-322.

Meyer, Deborah G. 'DoD Likely to Spend $250-Billion on C^{3}I Through 1990', Armed Forces Journal International, Vol. 122 (February 1985), 72-84.

Miles, Wyndham D. 'The Polaris' in Eugene M. Emme (ed.), The History of Rocket Technology: Essays on Research, Development, and Utility (Detriot: Wayne State University, 1964), 162-75.

Miller, Barry. 'Advanced Reentry Vehicle Tests Planned', Aviation Week & Space Technology, Vol. 104 (May 24, 1976), 22-3.

Miller, Barry. 'Radio Sextant Developed for Submarines', Aviation Week, Vol. 73 (February 29, 1960), 81, 83.

Miller, Barry. 'Studies of Penetration Aids Broadening', Aviation Week & Space Technology, Vol. 80 (January 20, 1964), 72-93.

Mintz, M. 'Depth Charge: Cost Overruns on New Trident Sub Leave a Muddied Wake', Washington Post (October 4, 1981), reprinted in Dina Rasor (ed.), 211-23.

Murphy, Charles J. V. 'US Sea Power: "The New Mix"', Fortune, Vol. 60 (August 1959), 76-83, 180-87.

Nelson, R. R. and S.G. Winter, 'In Search of a Useful Theory of Innovation', Research Policy, Vol. 6 (1977), 36-76.

Nitze, Paul H. 'Deterring our Deterrent', Foreign Policy Vol. 25 (Winter 1976/1977), 195-210.

Nitze, Paul H. 'Nitze Discusses Navy's Major Missions', Navy Magazine (July 1964), 27-30, 63-64.

Norris, Robert S. 'Counterforce at Sea: The Trident II Missile', Arms Control Today (September 1985), 5-10.

North, David M. 'Survivability Key to Trident Program', Aviation Week & Space Technology (June 16, 1980), 101-6.
Vol. 112

Pakenham, Captain W. T. T. 'The Command and Control of Submarine Operations', Naval Forces, Vol II (1985), 50-55.

Paolucci, D. A. 'Poseidon and Minuteman - Either, Or; Neither, Nor?', United States Naval Institute Proceedings (August 1968), 46-58.
Vol. 94

Paolucci, D. A. 'The Development of Navy Strategic Offensive and Defensive Systems', United States Naval Institute Proceedings, Vol. 96 (May 1970), 204-223.

Pinch, T. and W. Bijker, 'Science, Relativism and the New Sociology of Technology', Social Studies of Science, Vol. 16 (1986), 347-60.

Pinch, T. J. and W. E. Bijker, 'The Social Construction of Facts and Artefacts: or How the Sociology of Science and the Sociology of Technology might Benefit Each Other', Social Studies of Science, Vol. 14 (1984), 399-441.

Polmar, Norman and Captain D. A. Paolucci. 'Sea-Based "Strategic" Weapons for the 1980s and Beyond', United States Naval Institute Proceedings/Naval Review 1978, Vol. 104 (May 1978), 98-113.

Polmar, Norman. 'That Other Leg in the Triad', Air Force Magazine, (July 1985), 84-91.

Vol 28
Postol, Theodore A. 'The Trident and Strategic Stability', Oceanus (Summer 1985), 45-54.

Powers, Thomas. 'Choosing a Strategy for World War III', Atlantic (November 1982), 82-110

Raborn, Rear Admiral William F. 'Navy Within a Navy', Astronautics and Aeronautics, Vol. 10 (October 1972), 63-65.

Raborn, W. F. and John P. Craven. 'The Significance of Draper's Work in the Development of Naval Weapons' in S. Lees (ed.), Air, Space and Instruments: Draper Anniversary Volume (New York: McGraw-Hill, 1963), 15-31.

Reding, J. Peter and Dennis M. Jecman, 'An Advanced Aerosspike to Minimize Nose Drag', Lockheed Horizons (1984), 46-54.

Reed, William S. 'Lockheed Trims Polaris Vehicle's Weight', Aviation Week, Vol. 72 (February 8, 1960), 52-9.

Roberts, A. 'Preparing to Fight a Nuclear War', Arena, No. 57 (1981), 45-93.

Robinson, Clarence A. 'Congress Questioning Viability of MX ICBM', Aviation Week & Space Technology (March 22, 1982), 18-20.
Vol. 116

Robinson, Clarence A., Jr. 'New Propellant Evaluated For Trident Second Stage', Aviation Week & Space Technology (October 13, 1975), 15-9.
Vol. 103

Robinson, Clarence A., Jr. 'Senate Votes 1978 Trident Operation', Aviation Week & Space Technology, Vol. 99 (October 1, 1973), 16-17.

Robinson, Clarence A., Jr. 'Trident Post-Boost Control Tests Planned', Aviation Week & Space Technology Vol. 103 (October 27, 1975), 63-73.

Robinson, R. B. 'Polaris Navigation and the Gyroscope', US Naval Institute Proceedings, Vol. 91 (December 1965), 148-52.

Rosenberg, David Allen. '"A Smoking Radiating Ruin at the End of Two Hours" - Documents on American Plans for Nuclear War with the Soviet Union, 1954-1955', International Security, Vol. 6 (Winter 1981/82), 3-38.

Rosenberg, David Allen. 'American Atomic Strategy and the Hydrogen Bomb Decision', Journal of American History, Vol. 66 (1979), 62-87.

Rosenberg, David Allen. 'Arleigh Albert Burke' in Robert William Love, Jr. (ed.), The Chiefs of Naval Operation (Annapolis, Md.: Naval Institute Press, 1980), 263-319.

Rosenberg, David Allen. 'The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960', International Security, Vol. 7 (Spring 1983), 3-71.

Rosenberg, David Allen. 'US nuclear stockpile, 1945 to 1950', Bulletin of the Atomic Scientists, (May 1982), 25-30.

Russell, S. 'The Social Construction of Artefacts: A Response to Pinch and Bijker', Social Studies of Science, Vol. 16 (1986), 331-46.

Schroeer, Dietrich. 'Quantifying Technological Imperatives in the Arms Race', in D. Carlton and C. Schaerf, Reassessing Arms Control (Macmillan, 1985), 60-71.

Scoville, Herbert, Jr. 'Missile Submarines and National Security', Scientific American, Vol. 226 (June 1972), 15-27.

Seidenman, Paul. ' The Sea-Based Leg of the TRIAD', National Defense (December 1981), 43-46, 64.

Seppelin, T. O. 'The Department of Defense World Geodetic System 1972', The Canadian Surveyor, Vol. 28, No. 5 (December 1974), 496-506.

Shapley, Deborah. 'Arms Control as a Regulator of Military Technology', Daedalus, Vol. 109 (Winter 1980), 145-57.

Shapley, Deborah. 'Technology Creep and the Arms Race: ICBM Problem a Sleeper', Science, Vol. 201 (22 September 1978), 1102-1105.

Shapley, Deborah. 'Trident in Trouble: New Missile May Resemble Poseidon, After All', Science, Vol. 191 (January 9, 1976), 50-1.

Simon, H. 'A Behavioral Model of Rational Choice', Quarterly Journal of Economics Vol. 69 (1955), 99-118.

Siuru, William D. 'SLBM - the Navy's contribution to Triad', Air University Review, (September/October 1977), 17-29.

Slocombe, Walter B. 'Why We Need Counterforce at Sea', Arms Control Today (September 1985), 10-12.

Smith, Levering, Robert H. Wertheim & Robert A. Duffy. 'Innovative Engineering in the Trident Missile Development', The Bridge (National Academy of Engineering), Vol. 10 (1980), 10-19.

Smith, Robert W. and Joseph N. Tatarewicz, 'Replacing a Technology: the Large Space Telescope and CCDs', Proceedings of the IEEE Vol. 73 (1985), 1221-35.

Spaven, Malcolm. 'Communicating with submarines', Jane's Defence Weekly, Vol. 4 (November 23, 1985), 1152-56.

Spaven, Malcolm. 'ELF: Surviving the traumas - part 2', Jane's Defence Weekly, Vol. 4 (November 30, 1985), 1194-97.

Staff of the Space Analysis and Computation Group, 'Planned Improvements in the Transit System (1975)', Navigation: Journal of The Institute of Navigation, Vol. 22, No. 4 (Winter 1975-76), 352-60.

Stansell, Jr., T. A. 'The Many Faces of Transit', Navigation: Journal of The Institute of Navigation, Vol. 25, No. 1 (Spring 1978), 55-70.

Steinbruner, John and Barry Carter. 'Organizational and Political Dimensions of the Strategic Posture: The Problems of Reform', Daedalus, Vol. 104 (Summer 1975), 131-54; reprinted in Franklin A. Long and George W. Rathjens (eds.). Arms, Defense Policy, and Arms Control (New York: Norton, 1976), 131-54.

Steinmetz, Joseph N., Jr. 'Test Complex Simulates Polaris Re-entry Heat', Missiles and Rockets, Vol. 14 (January 6, 1964), 24-5.

Strunk, Ben. 'SLAG Makes Polaris Firings "Fail-Safe"', Missiles and Rockets, Vol. 8 (January 9, 1961), 23-24.

Thomas, B. K. 'Poseidon, Minuteman 3 Test Firings Begin', Aviation Week & Space Technology, Vol. 89 (August 23, 1968), 23.

Thompson, T. 'Performance of the Satrack/Global Positioning System Trident I Missile Tracking System', Proceedings of IEEE Position, Location and Navigation Symposium (December 1980), 445-49.

Trageser, M. B. 'Floated Gravity Gradiometer', IEEE Transactions on Aerospace and Electronic Systems Vol. 20 (1984), 417-19.

Tritten, Commander James John. 'TRIDENT: Strategic Navy Program', National Defense (April 1983), 40-43.

Ulsamer, Edgar. 'Toward A New World Strategy', Air Force Magazine (January 1979), 60-65.

Various, 'Polaris: Special Report', Missiles and Rockets, Vol. 7 (July 25, 1960).

Walter, R. B. 'The "Brain" of the Polaris Missile', Missiles and Rockets, Vol. 8 (June 12, 1961), 30-1, 53.

Whipple, G. G. 'Power for Polaris', Ordnance (January-February 1962), 583-5.

Whitmore, William F. 'Military Operations Research - A Personal Retrospective', Operations Research, Vol. 9 (March-April 1961).

Whitmore, William F. 'The Origins of Polaris', US Naval Insitute Proceedings Vol. 106 (March 1980), 56-59.

Wit, J. S. 'American SLBM: Counterforce Options and Strategic Implications', Survival Vol. 24 (1982), 163-74.

Yaffee, Michael. 'Ablation Wins Missile Performance Gain', Aviation Week Vol. 73 (July 18, 1960), 54-65.

Yaffee, Michael. 'Pyrolytic Graphite Studied for Re-Entry', Aviation Week Vol. 73 (July 25, 1960), 26-28.

Yanarella, Ernest J. 'The "Technological Imperative" and the Strategic Arms Race', Peace and Change, Vol. III (Spring 1975), 3-16.

Zuckerman, Lord. 'Science Advisers and Scientific Advisers', Proceedings of the American Philosophical Society, Vol. 124, No. 4 (August 1980), 241-55.

Unpublished Documents

Bruins, B. D. 'U.S. Naval Bombardment Missiles, 1940-1958: A Study of the Weapons Innovations Process' (Columbia University PhD Thesis, 1981).

Conigliaro, S. A. 'From Polaris to Trident Navigation', Paper presented to National Marine Meeting, Institute of Navigation, US Merchant Marine Academy, Kings Point, Long Island, New York, 23 October 1973.

Draper, C. S. 'Submarine Inertial Navigation - A Review and Some Predictions', Paper Presented to the Polaris Steering Task Group on October 22, 1959 (Draper Laboratory Library, CSD-107)

Greenwood, Ted. 'Qualitative Improvements in Offensive Strategic Arms: The Case of MIRV', (MIT Department of Political Science, PhD Thesis, August 1973).

Hunter, Robert E. 'Politics and Polaris: The Special Projects Office of the Navy as a Political Phenomenon', (Wesleyan University Senior Honours Thesis, June 1962).

Kurth, R. J. 'The Politics of Technological Innovation in the United States Navy' (Harvard University Dissertation, 1970).

Langford, R. C. 'Unconventional Inertial Sensors', paper presented to Second American Institute of Aeronautics and Astronautics Annual Meeting, San Francisco, 26-29 July 1965.

Naval Warfare Analysis Group Study No. 1, 'Introduction of the Fleet Ballistic Missile into Service' (January 1957), Serial 007P93.

Olson, Benedict O. 'History of FBM Guidance at CSDL' (Typescript: March 10, 1975).

Spaven, Malcom. 'Extremely Low Frequency Communications for Submarines: A Background Briefing on British Plans', (Brighton: Armament and Disarmament Unit, University of Sussex, Second Edition; January 1986).

Sidrow, M. R. 'Politics and Military Weapons Acquisition: The Limits of Bureaucratic Political Theory' (PhD thesis, University of California, Riverside, 1983).

Strategic Systems Program Office, 'A Programmatic History of Trident II' (Washington, DC: SSPO, November 1982).

Topping, Captain Robert L. 'Submarine Launched Ballistic Missile Improved Accuracy', American Institute of Aeronautics and Astronautics, 1981 Annual Meeting and Technical Display (AIAA-81-0935).

Weapon System Evaluation Group Report No. 23, 'The Relative Military Advantages of Missiles and Manned Aircraft' (May 6, 1957).

Wheaton, Graydon M. 'Electronics Manufacturing For Inertial Guidance Systems' (Typescript: May 9, 1986).

Government Publications

In addition to the documents listed here I have also made extensive use of Congressional hearings. Particularly useful are those from the Research and Development Subcommittee of the Senate Armed Services Committee during the 1970s.

Congressional Budget Office, Trident II Missile Test Program (Washington, DC: CBO Staff Working Paper, February 1986).

Congressional Budget Office, Trident II Missiles: Capability, Costs, and Alternatives (Washington, DC: CBO, July 1986).

Institute for Defense Analysis. Strat-X - Volume 1: The Strat-X Report (Arlington, Virginia: Institute for Defense Analysis, August 1967); declassified, sanitized 1971.

Special Projects Office. The Polaris Fleet Ballistic Missile Weapons System, (Washington, DC: SPO, August 1960).

Strategic Systems Program Office, FBM facts/chronology - Polaris, Poseidon, Trident (Washington, DC: Navy Department, 1986), and earlier editions.

Industry Publications

Carlson, R. W. and C. A. Louis III, Introduction to Re-entry Flight Dynamics (Sunnyvale, Calif.: Lockheed Missiles & Space Co., Inc., March 15, 1968).

Lockheed Missiles & Space Co., Inc. Fleet Ballistic Missiles - 25 Years (Sunnyvale, Calif.: Lockheed Missiles & Space Co., Inc., n.d.)

Lockheed Missiles & Space Co., Inc. Trident II (D5) Fleet Ballistic Missile Fact Sheet (Sunnyvale, Calif.: Lockheed Missiles & Space Co., Inc., n.d.)

Lockheed Missiles & Space Division. 'Special Polaris Report', Trajectory (Winter 1960-61).

List of Interviewees

Vice Admiral (US Navy, Retd.) Levering Smith. San Diego, March 31, April 1 & 2, 1987.

Captain (US Navy, Retd.) Louis Shock. San Diego, April 1, 1987.

Rear Admiral (US Navy, Retd.) N. G. Ward. San Diego, April 1, 1987.

Dr Herbert York. San Diego, April 2, 1987.

Rear Admiral (US Navy Retd.) Robert Wertheim. Calabasas, Los Angeles, April 3, 1987.

Robert Aldridge. Santa Clara, April 4, 1987.

Dr Willy Fiedler. Los Altos Hills, April 4, 1987.

Dr George Mechlin. Pittsburgh, April 8, 1987.

Rear Admiral (US Navy Retd.) Harvey Lyon. Arlington, Virginia, April 21, 1987.

Eliott Mitchell. National Air and Space Museum, Washingon, DC, April 27, 1987.

Captain (US Navy Retd.) Grayson Merrill. San Diego, May 4, 1987.

Dr Robert Fuhrman. Calabasas, Los Angeles, May 5, 1987.

Dr Werner Kirchner. San Dimas, Los Angeles, May 5, 1987.

Carl Haussmann. Cupertino, California, May 5, 1987.

Dr Derald Stuart. Sunnyvale, California, May 6 & 8, 1987.

Chet Zimmerman. Sunnyvale, May 6, 1987.

Ted Postol. Stanford University, May 7, 1987.

Dave Montague. Sunnyvale, May 8, 1987.

David Nixon. Sunnyvale, May 8, 1987.

Dr William Whitmore. Los Altos Hills, California, May 9, 1987.

Ben Olson. Cambridge, Massachusetts, May 11, 1987.

Graydon Wheaton. Cambridge, Massachusetts, May 11, 1987.

Robert Duffy. Cambridge, Massachusetts, May 11, 1987.

David Hoag. Cambridge, Massachusetts, May 11, 1987.

Larry Smith. Cambridge, Massachusetts, May 14, 1987.

Sam Forter. Cambridge, Massachusetts, May 15, 1987.

Sanford Cohen. Cambridge, Massachu sets, May 15, 1987.

Paul Dow. Cambridge, Massachu sets, May 15, 1987.

Sam Claypoole & Hyman Strell. Long Island, New York, May 18, 1987.

Vice Admiral Glenwood Clark. Arlington, Virginia, May 21, 1987.

John Brett. National Air and Space Museum, May 28, 1987.

James Martin. McLean, Virginia, June 9, 1987.

Dave Gold. National Air and Space Museum, June 10, 1987.

Phil Faurot & Ron Kranz. Arlington, Virginia, June 12, 1987.

John Coyle. Washington, DC, June 17, 1987.

Joe Cestone. Arlington, Virginia, June 22, 1987.

Dr Seymour Zeiberg. Washington, DC, June 27, 1987.

Dr. Richard DeLauer. Arlington, Virginia, June 30, 1987.

Rear Admiral (US Navy, Retd.) Ross Williams. Arlington, Virginia, July 7, 1987.

Captain (US Navy, Retd.) Steven Cohen. Virginia, July 9, 1987.

Vice Admiral (US Navy, Retd.) Robert Y. Kaufman. Potomac, Maryland, July 10, 1987.

Dr Alexander Kossiakoff. Silver Spring, Maryland, July 13, 1987.

Sam Sugg & Andy Andrews. Silver Spring, Maryland, July 13, 1987.

Bob Jenkins. Silver Spring, Maryland, July 13, 1987.

Dr Marvin Stern. Washington, DC, July 13, 1987.

Jack Fagin. Arlington, Virginia, July 14, 1987.

Interviews conducted by Donald MacKenzie

Ralph Ragan. Cambridge, Mass., October 4, 1984.

Edward J. Hall. Cambridge, Mass., October 4, 1984.

Paul Dow & Ben Olson. Cambridge, Mass., October 5, 1984.

David Hoag. Cambridge, Mass., November 5, 1984.

Vice Admiral (US Navy, Retd.) Levering Smith. San Diego, February 23, 1985.

Rear Admiral (US Navy, Retd.) Robert Wertheim. Burbank, Los Angeles, March 4, 1985.

Morris Kuritsky, Murray Goldstein & Ed Solov. New Jersey, March 18, 1985.

Andrew DePrete. Arlington, Virginia, March 28, 1985.

Vice Admiral Glenwood Clark. Arlington, Virginia, March 28, 1985.

Robert Mitchell and Clifton Chappel. Arlington, Virginia, March 29, 1985.

Captain Steven Cohen. Arlington, Virginia, March 29, 1985.

Thomas A. J. King. Arlington, Virginia, April 2, 1985.

Larry Smith. Cambridge, Mass., April 10, 1985.